

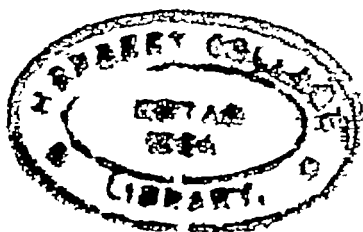
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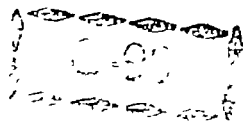
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WONDER BOOK
OF THE
WORLD'S PROGRESS
VOL. V
HERO TALES • PLANTS



WONDER BOOK OF THE WORLD'S PROGRESS

By
HENRY SMITH WILLIAMS

IN TEN VOLUMES
Illustrated

•

VOLUME V
Hero Tales
Plants



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INTRODUCTION

THE word hero conveys different meanings to different minds. The popular conception of a hero is a person who performs deeds involving bravery—a risking of life. The accomplishment may be on an heroic scale, as with Alexander the Great, Julius Cæsar, Napoleon, or it may be the accomplishment of an individual who risked his life for some purpose other than bravado. There are legendary heroes without number that fall within this class; and no one needs to be reminded of such contemporary names as those of Commander Hobson, Sergeant York, and Colonel Lindbergh.

Our present purpose, however, contemplates the observation of heroes of a somewhat different order. Since we must choose among the multitudes, it is consonant with the character of the present work to observe the lives of certain men whose bravery might be said to be intellectual rather than physical. The deeds of traditional heroes of history and romance were not necessarily associated with matters of actual or vital importance to their contemporaries or to posterity. In the last analysis, what we applaud and envy in their achievements is their exhibition of willingness to put their own personal safety—their lives—against a certain practical purpose. If the purpose be not attained, the hero is not remembered as such—his name passes into oblivion. If, on the other hand, the heroic purpose terminates in achievement, the name and fame of the performer may outrival that of all other men of his generation.

To give a practical illustration of what scarcely needs illustrating, one need only recall the demonstration accorded Colonel Lindbergh on that memorable day when he returned to New York. On that occasion, if memory serves me, the leading metropolitan daily newspaper gave no fewer than seventeen entire pages to an account of his triumph—part of a popular acclaim that had scarcely had a parallel since the days when Imperial Rome was wont to go mad over the return of a citizen whose feats of arms had annexed vast new territories and made subject entire nations of people.

It is indeed well that humanity should accord such recognition to heroes of this traditional type. After all, in the last analysis, courage is everything—or nearly everything. The reading of hero tales is to be commended to every youth. Had we space for it here, I should like to cite at least a score of ancient, medieval, and modern heroes whose individual deeds or military leadership have made their names legendary. But since a choice must be made, I shall adopt the less conventional expedient of presenting a coterie of men who may or may not have possessed physical courage, but whose intellectual courage led them into frontiers of the world of thought.

Such frontiers always constitute controversial territory. And the man who enters them is never regarded as a hero by his contemporaries. For him there is no applause of the multitudes. At best he labors obscurely, unrecognized and therefore undisturbed. At worst, he arouses the antagonism of persons in authority, whose traditional stand is to uphold the past rather than to look to the future. In that event he meets opposition which may be intellectual, but on occasion may become physical as well.

Witness a Socrates drinking the hemlock; a Servetus at the stake; a Lavoisier at the guillotine.

In general, however, it is obscurity rather than persecutorial publicity that attends the intellectual pioneer. Nor is it necessary, or perhaps desirable, to dwell over-much on this aspect of the lives of the pioneer workers in question. It suffices rather to recall their achievements, as recognized and interpreted by posterity.

To turn from generals to particulars, the men I have in mind are the scientific investigators whose researches have resulted in the development of new principles, or in the observation of new facts which were in due course instrumental in changing essential aspects of civilization. There may be nothing "heroic" about the lives of these men in the conventional sense, as already suggested. Yet the name justly attaches to them because of the extraordinary character of the intellectual and practical developments that grew out of their efforts.

The contrast in this regard is very noticeable. Heroes of the other type, even those of immortal fame — an Alexander, a Cæsar, a Napoleon — while making a tremendous impression on their generation and becoming legendary figures for all posterity, may nevertheless have failed greatly to modify the civilization of epochs succeeding their own. But it is the very essence of the heroism of the scientific discoverer that his work does ultimately advance civilization.

Indeed, it is hardly too much to say that every great forward step in civilization is conditioned on some scientific discovery, and could not have been attained without that discovery.

Otherwise stated, scientific progress is the foundation of all progress of civilization.

That thesis will perhaps seem better substantiated

after the lives of a series of scientific heroes have been passed in review. To give coherence to the story, I have selected, first, a group of extraordinary men who were in the field toward the close of the eighteenth century; then successive groups of contemporaries working in different fields, who carried forward the conquest of science through the nineteenth century.

The workers whose specific efforts are followed are those who pioneered the physical, the chemical, and the biological sciences. But as background for the presentation, we have a picture gallery of other contemporary workers, together with suggestions of the developments in practical or applied science, which were naturally sequential to the discovery and development of scientific principles by the devotees of what is commonly spoken of as "pure science."

As a whole, then, we have in this volume what might be described as an epitome of the development of certain important aspects of nineteenth-century science. The names of the workers who were responsible for that development were only by exception publicly acclaimed on anything like an equality with the names of political and military heroes. But by posterity they will be well remembered long after most other names of their contemporaries are forgotten.

That, however, is a matter of no special consequence. What is of consequence is the fact, which I reiterate for emphasis, that the deeds of these silent and often obscure workers are vastly more important for the progress of humanity than the political and military accomplishments which take chief place in contemporary records and often in the pages of history.



FREDERICK WILLIAM HERSCHEL

SCIENCE AT THE BEGINNING OF THE
NINETEENTH CENTURY

IN the year 1896 word came out of Germany of a scientific discovery that startled the world. It came first as a rumor, little credited; then as a pronounced report; at last as a demonstration. It told of a new manifestation of energy, in virtue of which the interior of opaque objects is made visible to human eyes.

One had only to look into a tube containing a screen of a certain composition, and directed toward a peculiar electrical apparatus, to acquire clairvoyant vision more wonderful than the discredited second sight of the medium. Coins within a purse, nails driven into wood, spectacles within a leather case, became clearly visible when subjected to the influence of this magic tube; and when a human hand was held before the tube, its bones stood revealed in weird simplicity, as if the living, palpitating flesh about them were but the shadowy substance of a ghost.

Not only could the human eye see these astounding revelations, but the impartial evidence of inanimate chemicals could be brought forward to prove that the mind harbored no illusion. The photographic film recorded the things that the eye might see, and ghostly pictures galore soon gave a quietus to the doubts of the most skeptical. Within a month of the announcement of Professor Röntgen's experiments comment upon the

"X ray" and the "new photography" had become a part of the current gossip of all Christendom.

It was but natural that thoughtful minds should have associated this discovery of our boasted latter-day epoch with another discovery made in the earliest infancy of that century. In the year 1801 Thomas Wedgwood, of the world-renowned family of potters, and Humphry Davy, the youthful but already famous chemist, made experiments which showed that it was possible to secure the imprint of a translucent body upon a chemically prepared plate by exposure to sunlight.

In this way translucent pictures were copied, and skeletal imprints were secured of such objects as leaves and the wings of insects—imprints strikingly similar to the "shadowgraphs" of more opaque objects which we secure by means of the "new photography" today. But these experimenters little dreamed of the real significance of their observations. It was forty years before practical photography, which these observations foreshadowed, was developed and made of any use outside the laboratory.

It seems strange enough now that imaginative men—and Davy surely was such a man—should have paused on the very brink of so great a discovery. But to harbor that thought is to misjudge the nature of the human mind. Things that have once been done seem easy; things that have not been done are difficult, tho they lie but a hair's-breadth off the beaten track. Who can today foretell what revelations may be made, what useful arts developed, forty years hence through the agency of the radio or the motion picture?

It is no part of my purpose, however, to attempt the impossible feat of casting a horoscope for the new photography. My present theme is reminiscent, not pro-

phetic. I wish to recall what knowledge of the sciences men had in the days when that discovery of Wedgwood and Davy was made, a little more than a hundred years ago; to inquire what was the scientific horizon of a person standing at the threshold of the nineteenth century.

Let us glance briefly at each main department of the science of that time, that we may know whither men's minds were trending in those closing days of the eighteenth century, and what were the chief scientific legacies of that century to its successor.

In the field of astronomy the central figure during this closing epoch of the eighteenth century is William Herschel, the Hanoverian, whom England has made hers by adoption. He is a man with a positive genius for sidereal discovery. At first a mere amateur in astronomy, he snatches time from his duties as music-teacher to grind him a telescopic mirror, and begins gazing at the stars. Not content with his first telescope, he makes another, and another, and he has such genius for the work that he soon possesses a better instrument than was ever made before.

His patience in grinding the curved reflective surface is monumental. Sometimes for sixteen hours together he must walk steadily about the mirror, polishing it, without once removing his hands. Meantime his sister, always his chief lieutenant, cheers him with her presence, and from time to time puts food into his mouth.

The telescope completed, the astronomer turns night into day, and from sunset to sunrise, year in and year out, sweeps the heavens unceasingly, unless prevented by clouds or the brightness of the moon. His sister sits always at his side, recording his observations. They are in the open air, perched high at the mouth of the reflector, and sometimes it is so cold that the ink freezes in

the bottle in Caroline Herschel's hand; but the two enthusiasts hardly notice a thing so commonplace as terrestrial weather. They are living in distant worlds.

The results? What could they be? Such enthusiasm would move mountains. But, after all, the moving of mountains seems a Lilliputian task compared with what Herschel really does with those wonderful telescopes. He moves worlds, stars, a universe—even, if you please, a galaxy of universes, at least he proves that they move, which seems scarcely less wonderful; and he expands the cosmos, as man conceives it, to thousands of times the dimensions it had before. As a mere beginning, he doubles the diameter of the solar system by observing the great outlying planet which we now call Uranus, but which he christens *Georgium Sidus*, in honor of his sovereign, and which his French contemporaries, not relishing that name, prefer to call Herschel.

This discovery is but a trifle compared with what Herschel does later on, but it gives him world-wide reputation none the less. Comets and moons aside, this is the first addition to the solar system that has been made within historic times, and it creates a veritable furor of popular interest and enthusiasm. Incidentally King George is flattered at having a world named after him, and he smiles on the astronomer, and comes with his court to have a look at his namesake. The inspection is highly satisfactory; and presently the royal favor enables the astronomer to escape the thralldom of teaching music, and to devote his entire time to the more congenial task of star-gazing.

Thus relieved from the burden of mundane embarrassments, he turns with fresh enthusiasm to the skies, and his discoveries follow one another in bewildering profusion. He finds various hitherto unseen moons of

our sister planets; he makes special studies of Saturn, and proves that this planet, with its rings, revolves on its axis; he scans the spots on the sun, and suggests that they influence the weather of our earth; in short, he extends the entire field of solar astronomy.

But very soon this field becomes too small for him, and his most important researches carry him out in the regions of space compared with which the span of our solar system is a mere point. With his perfected telescopes he enters abysmal vistas which no human eye ever penetrated before, which no human eye had hitherto more than vaguely imagined. He tells us that his forty-foot reflector will bring him light from a distance of "at least eleven and three-fourths millions of millions of millions of miles"—light which left its source two million years ago. The smallest stars visible to the unaided eye are those of the sixth magnitude; this telescope, he thinks, has power to reveal stars of the 1342d magnitude.

But what does Herschel learn regarding these awful depths of space and the stars that people them? That is what the world wishes to know. Copernicus, Galileo, Kepler, have given us a solar system, but the stars have been a mystery. What says the great reflector—are the stars points of light, as the ancients taught, and as more than one philosopher of the eighteenth century has still contended, or are they suns, as others hold?

Herschel answers, they are suns, each and every one of all the millions—suns, many of them, larger than the one that is the center of our tiny system. Not only so, but they are moving suns. Instead of being fixed in space, as has been thought, they are whirling in gigantic orbits about some common center. Is our sun that center? Far from it. Our sun is only a star, like all the rest, circling on with its attendant satellites—our giant sun a star, no

different from myriad other stars, not even so large as some; a mere insignificant spark of matter in an infinite shower of sparks.

Nor is this all. Looking beyond the few thousand stars that are visible to the naked eye, Herschel sees series after series of more distant stars, marshaled in galaxies of millions; but at last he reaches a distance beyond which the galaxies no longer increase. And yet—so he thinks—he has not reached the limits of his vision. What then? He has come to the bounds of the sidereal system; seen to the confines of the universe. He believes that he can outline this system, this universe, and prove that it has the shape of an irregular globe, oblately flattened to almost disk-like proportions, and divided at one edge—a bifurcation that is revealed even to the naked eye in the forking of the Milky Way.

This, then, is our universe as Herschel conceives it—a vast galaxy of suns, held to one center, revolving, poised in space. But even here those marvelous telescopes do not pause. Far, far out beyond the confines of our universe, so far that the awful span of our own system might serve as a unit of measure, are revealed other systems, other universes, like our own, each composed, as he thinks, of myriads of suns, clustered like our galaxy into an isolated system—mere islands of matter in an infinite ocean of space.

So distant from our universe are these new universes of Herschel's discovery that their light reaches us only as a dim nebulous glow, in most cases invisible to the unaided eye. About a hundred of these nebulae were known when Herschel began his studies. Before the close of the century he had discovered about two thousand more of them, and many of these had been resolved by his largest telescopes into clusters of stars. He believed

that the farthest of these nebulæ that he could see was at least 300,000 times as distant from us as the nearest fixed star. Yet that nearest star is so remote that its light, traveling 180,000 miles a second, requires three and one-half years to reach our planet.

As if to give the finishing touches to this novel scheme of cosmology, Herschel, tho in the main very little given to unsustained theorizing, allows himself the privilege of one belief that he cannot call upon his telescopes to substantiate. He thinks that all the myriad suns of his numberless systems are instinct with life in the human sense. Giordano Bruno and a long line of his followers had held that some of our sister planets may be inhabited, but Herschel extends the thought to include the moon, the sun, the stars—all the heavenly bodies.

He believes that he can demonstrate the habitability of our own sun, and reasoning from analogy, he is firmly convinced that all the suns of all the systems are "well supplied with inhabitants." In this, as in some other inferences, Herschel is misled by the faulty physics of his time. Future generations, working with perfected instruments, may not sustain him all along the line of his observations even, let alone his inferences. But how one's egotism shrivels and shrinks as one grasps the import of his sweeping thoughts!

Continuing his observations of the innumerable nebulæ, Herschel is led presently to another curious speculative inference. He notes that some star groups are much more thickly clustered than others, and he is led to infer that such varied clustering tells of varying ages of the different nebulæ. He thinks that at first all space may have been evenly sprinkled with the stars, and that the grouping has resulted from the action of gravitation. Looking forward, it appears that the time must come

when all the suns of a system will be drawn together and destroyed by impact at a common center. Already, it seems to him, the thickest clusters have "outlived their usefulness," and are verging toward their doom.

But again, other nebulae present an appearance suggestive of an opposite condition. They are not resolvable into stars, but present an almost uniform appearance throughout, and are hence believed to be composed of a shining fluid, which in some instances is seen to be condensed at the center into a glowing mass. In such a nebula Herschel thinks he sees a sun in process of formation.

Taken together, these two conceptions outline a majestic cycle of world formation and world destruction—a broad scheme of cosmogony, such as had been vaguely adumbrated two centuries before by Kepler, and in more recent times by Wright and Kant and Swedenborg. This so-called "nebular hypothesis" assumes that in the beginning all space was uniformly filled with cosmic matter in a state of nebular or "fire-mist" diffusion, "formless and void." It pictures the condensation—coagulation, if you will—of portions of this mass to form segregated masses, and the ultimate development out of these masses of the sidereal bodies which we see. Thus far the mind follows readily; but now come difficulties. How happens it, for example, that the cosmic mass from which was born our solar system was divided into several planetary bodies instead of remaining a single mass? Were the planets struck off from the sun by the chance impact of comets, as Buffon has suggested? or thrown out by explosive volcanic action, in accordance with the theory of Dr. Darwin? or do they owe their origin to some unknown law? In any event, how chanced it that all were projected in nearly the same plane?



PIERRE SIMON LAPLACE

It remained for a mathematical astronomer to solve these puzzles. The man of all others competent to take the subject in hand was the French astronomer Laplace. For a quarter of a century he had devoted his transcendent mathematical abilities to the solution of problems of motion of the heavenly bodies. Working in friendly rivalry with his countryman Lagrange, his only peer among the mathematicians of the age, he had taken up and solved one by one the problems that Newton left obscure.

Largely through the efforts of these two men the last lingering doubts as to the validity of the Newtonian hypothesis of universal gravitation had been removed. The share of Lagrange was hardly less than that of his co-worker; but Laplace will longer be remembered, because he ultimately brought his completed labors into a system, and incorporating with them the labors of his contemporaries, produced in the *Mécanique Céleste* the undisputed mathematical monument of the century, a fitting complement to the *Principia* of Newton, which it supplements and in a sense completes.

In the closing years of the century Laplace takes up the nebular hypothesis of cosmogony, to which we have just referred, and gives it definitive proportions; in fact, makes it so thoroughly his own that posterity will always link it with his name. Discarding the crude notions of cometary impact and volcanic eruption, Laplace fills up the gaps in the hypothesis with the aid only of well-known laws of gravitation and motion.

He assumes that the primitive mass of cosmic matter which was destined to form our solar system was revolving on its axis even at a time when it was still nebular in character, and filled all space to a distance far beyond the present limits of the system. As this vapor-

ous mass contracted through loss of heat, it revolved more and more swiftly, and from time to time, through balance of forces at its periphery, rings of its substance were whirled off and left revolving there, subsequently to become condensed into planets, and in their turn whirl off minor rings that became moons. The main body of the original mass remains in the present as the still contracting and rotating body which we call the sun.

The nebular hypothesis thus given detailed completion by Laplace is a worthy complement of the grand cosmologic scheme of Herschel. Whether true or false, the two conceptions stand as the final contributions of the eighteenth century to the history of man's ceaseless efforts to solve the mysteries of cosmic origin and cosmic structure.

The world listens eagerly and without prejudice to the new doctrines; and that attitude tells of a marvelous intellectual growth of our race. Mark the transition. In the year 1600, Bruno was burned at the stake for teaching that our earth is not the center of the universe. In 1700, Newton was pronounced "impious and heretical" by a large school of philosophers for declaring that the force which holds the planets in their orbits is universal gravitation. In 1800, Laplace and Herschel are honored for teaching that gravitation built up the system which it still controls; that our universe is but a minor nebula, our sun but a minor star, our earth a mere atom of matter, our race only one of a myriad races peopling an infinity of worlds. Doctrines which but the span of two human lives before would have brought their enunciators to the stake were now pronounced not impious, but sublime.

The eighteenth-century philosopher made great

strides in his studies of the physical properties of matter, and the application of these properties in mechanics, as the steam-engine, the balloon, the optic telegraph, the spinning-jenny, the cotton-gin, the chronometer, the perfected compass, the Leyden jar, the lightning-rod, and a host of minor inventions testify. In a speculative way he had thought out more or less tenable conceptions as to the ultimate nature of matter, as witness the theories of Leibnitz and Boscovich and Davy, to which we may recur.

But he had not as yet conceived the notion of a distinction between matter and energy, which is so fundamental to the physics of a later epoch. He did not speak of heat, light, electricity, as forms of energy or "force"; he conceived them as subtile forms of matter—as highly attenuated yet tangible fluids, subject to gravitation and chemical attraction; tho he had learned to measure none of them but heat with accuracy, and this one he could test only within narrow limits until late in the century, when Josiah Wedgwood, the famous potter, taught him to gage the highest temperatures with the clay pyrometer.

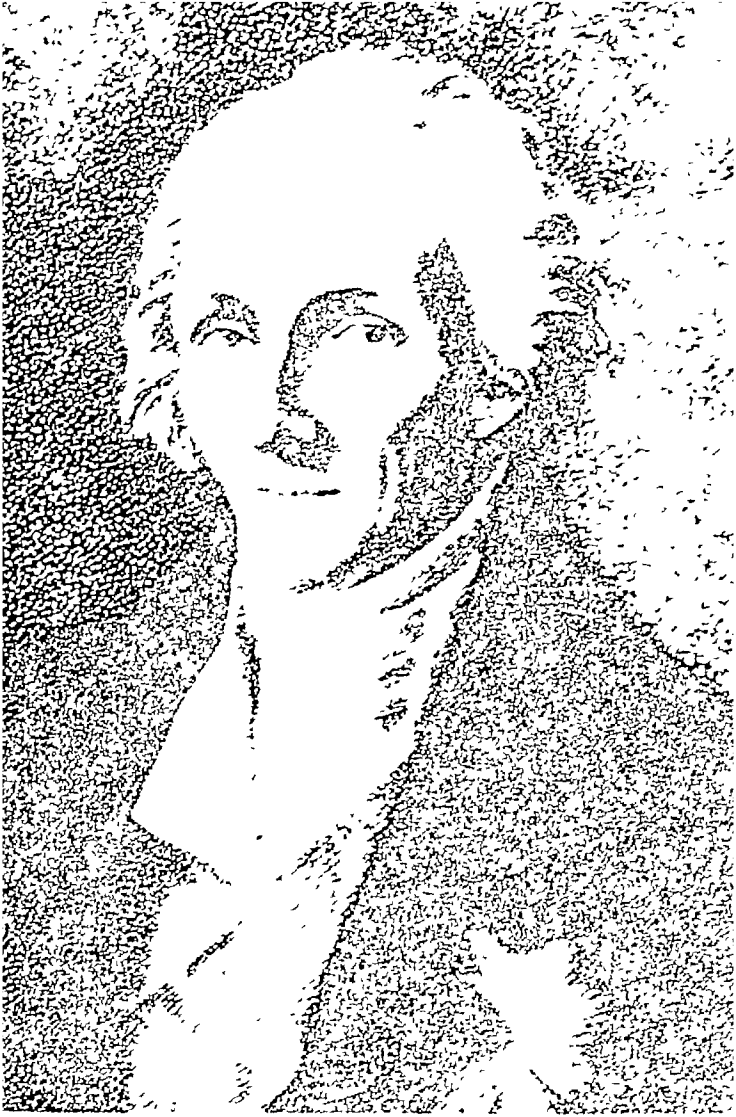
He spoke of the matter of heat as being the most universally distributed fluid in nature; as entering in some degree into the composition of nearly all other substances; as being sometimes liquid, sometimes condensed or solid, and as having weight that could be detected with the balance. Following Newton, he spoke of light as a "corpuscular emanation" or fluid, composed of shining particles which possibly are transmutable into particles of heat, and which enter into chemical combination with the particles of other forms of matter. Electricity he considered a still more subtile kind of matter—perhaps an attenuated form of light. Magnetism,

"vital fluid," and by some even a "gravic fluid," and a fluid of sound, were placed in the same scale; and taken together, all these supposed subtle forms of matter were classed as "imponderables."

This view of the nature of the "imponderables" was in some measure a retrogression, for many seventeenth-century philosophers, notably Hooke and Huygens and Boyle, had held more correct views; but the materialistic conception accorded so well with the eighteenth-century tendencies of thought that only here and there a philosopher, like Euler, called it in question, until well on toward the close of the century. Current speech referred to the materiality of the "imponderables" unquestioningly. Students of meteorology—a science that was just dawning—explained atmospheric phenomena on the supposition that heat, the heaviest imponderable, predominated in the lower atmosphere, and that light, electricity, and magnetism prevailed in successively higher strata. And Lavoisier, the most philosophical chemist of the century, retained heat and light on a par with oxygen, hydrogen, iron, and the rest, in his list of elementary substances.

But just at the close of the century the confidence in the status of the imponderables was rudely shaken in the minds of philosophers by the revival of the old idea of Fra Paolo and Bacon and Boyle, that heat, at any rate, is not a material fluid, but merely a mode of motion or vibration among the particles of "ponderable" matter.

The new champion of the old doctrine as to the nature of heat was a very distinguished philosopher and diplomatist of the time, who, it may be worth recalling, was an American. He was a sadly expatriated American, it is true, as his name, given all the official appendages, will amply testify; but he had been born and reared in a



Massachusetts village none the less, and he seems always to have retained a kindly interest in the land of his nativity, even tho he lived abroad in the service of other powers during all the later years of his life, and was knighted by England, ennobled by Bavaria, and honored by the most distinguished scientific bodies of Europe.

The American, then, who championed the vibratory theory of heat, in opposition to all current opinion, in this closing era of the eighteenth century, was Lieutenant-General Sir Benjamin Thompson, Count Rumford, F. R. S.

Rumford showed that heat may be produced in indefinite quantities by friction of bodies that do not themselves lose any appreciable matter in the process, and claimed that this proves the immateriality of heat. Later on he added force to the argument by proving, in refutation of the experiments of Bowditch, that no body either gains or loses weight in virtue of being heated or cooled. He thought it proved that heat is only a mode of motion.

But contemporary judgment, while it listened respectfully to Rumford, was little minded to accept his verdict. The cherished beliefs of a generation are not to be put down with a single blow. Where many minds have a similar drift, however, the first blow may precipitate a general conflict; and so it was here. Young Humphry Davy had duplicated Rumford's experiments, and reached similar conclusions; and soon others fell into line. Then, in 1800, Dr. Thomas Young—"Phenomenon Young" they called him at Cambridge, because he was reputed to know everything—took up the cudgels for the vibratory theory of light, and it began to be clear that the two "imponderables," heat and light, must stand or fall together; but no one as yet made a claim against the fluidity of electricity.

But before this speculative controversy over the nature of the "imponderables" had made more than a fair beginning, in the last year of the century, a discovery was announced which gave a new impetus to physical science, and for the moment turned the current of speculation into another channel. The inventor was the Italian scientist Volta; his invention, the apparatus to be known in future as the voltaic pile—the basis of the galvanic battery.

Ten years earlier Galvani had discovered that metals placed in contact have the power to excite contraction in the muscles of animals apparently dead. Working along lines suggested by this discovery, Volta developed an apparatus composed of two metals joined together and acted on by chemicals, which appeared to accumulate or store up the galvanic influence, whatever it might be. The effect could be accentuated by linking together several such "piles" into a "battery."

This invention took the world by storm. Nothing like the enthusiasm it created in the philosophic world had been known since the invention of the Leyden jar, more than half a century before. Within a few weeks after Volta's announcement, batteries made according to his plan were being experimented with in every important laboratory in Europe. The discovery was made in March. Early in May two Englishmen, Messrs. Nicholson and Carlyle, practising with the first battery made in their country, accidentally discovered the decomposition of water by the action of the pile. And thus in its earliest infancy the new science of "galvanism" had opened the way to another new science—electro-chemistry.

As the century closed, half the philosophic world was speculating as to whether "galvanic influence" were a new imponderable or only a form of electricity; and the

other half was eagerly seeking to discover what new marvels the battery might reveal. The least imaginative man could see that here was an invention that would be epoch-making, but the most visionary dreamer could not even vaguely adumbrate the real measure of its importance. Hitherto electricity had been only a laboratory aid or a toy of science, with no suggestion of practical utility beyond its doubtful application in medicine; in future, largely as the outgrowth of Volta's discovery, it was destined to become a great economic agency, whose limitations not even the enlarged vision of our later century can pretend to outline.

Of all the contests that were waging in the various fields of science in this iconoclastic epoch, perhaps the fiercest and most turbulent was that which fell within the field of chemistry. Indeed, this was one of the most memorable warfares in the history of polemics. It was a battle veritably Napoleonic in its inception, scope, and incisiveness. As was fitting, it was a contest of France against the world; but the Napoleonic parallel fails before the end, for in this case France won not only speedily and uncompromisingly, but for all time.

The main point at issue concerned the central doctrine of the old chemistry—the doctrine of Becher and Stahl, that the only combustible substance in nature is a kind of matter called phlogiston, which enters into the composition of other bodies in varying degree, thus determining their inflammability. This theory seems crude enough now, since we know that phlogiston was a purely fictitious element, yet it served an excellent purpose when it was propounded and it held its place as the central doctrine of chemical philosophy for almost a century.

At the time when this theory was put forward, it



must be recalled, the old Aristotelian idea that the four primal elements are earth, air, fire, and water still held sway as the working foundation of all chemical philosophies. Air and water were accepted as simple bodies. Only a few acids and alkalies were known, and these but imperfectly; and the existence of gases as we now know them, other than air, was hardly so much as suspected. All the known facts of chemistry seemed then to harmonize with the phlogiston hypothesis; and so, later on, did the new phenomena which were discovered in such profusion during the third quarter of the eighteenth century—the epoch of pneumatic chemistry.

Hydrogen gas, discovered by Cavendish in 1776, and called inflammable air, was thought by some chemists to be the very principle of phlogiston itself. Other "airs" were adjudged "dephlogisticated" or "phlogisticated" in proportion as they supported or failed to support combustion. The familiar fact of a candle flame going out when kept in a confined space of ordinary air was said to be due to the saturation of this air with phlogiston. And all this seemed to tally beautifully with the prevailing theory.

But presently the new facts began, as new facts always will, to develop an iconoclastic tendency. The phlogiston theory had dethroned fire from its primacy as an element by alleging that flame is due to a union of the element heat with the element phlogiston. Now earths were decomposed, air and water were shown to be compound bodies, and at last the existence of phlogiston itself was to be called in question. The structure of the old chemical philosophy had been completely riddled; it was now to be overthrown.

The culminating observation which brought matters to a crisis was the discovery of oxygen, which was made



HENRY CAVENDISH



by Priestley in England and Scheele in Sweden, working independently, in the year 1774. Priestley called the new element "dephlogisticated air"; Scheele called it "empyreal air."

But neither Priestley nor Scheele realized the full import of this discovery; nor, for that matter, did any one else at the moment. Very soon, however, one man at least had an inkling of it. This was the great French chemist Antoine Laurent Lavoisier. It has sometimes been claimed that he himself discovered oxygen independently of Priestley and Scheele. At all events, he at once began experimenting with it, and very soon it dawned upon him that this remarkable substance might furnish a key to the explanation of many of the puzzles of chemistry.

Lavoisier found that oxygen is consumed or transformed during the combustion of any substance in air. He reviewed the phenomena of combustion in the light of this new knowledge. It seemed to him that the new element explained them all without aid of the supposititious element phlogiston. What proof, then, have we that phlogiston exists? Very soon he is able to answer that there is no proof, no reason to believe that it exists. Then why not denounce phlogiston as a myth, and discard it from the realm of chemistry?

Precisely this is what Lavoisier purposes to do. He associates with him three other famous French chemists, Berthollet, Guyton de Morveau, and Fourcroy, and sets to work to develop a complete system of chemistry based on the new conception. In 1788 the work is completed and given to the world. It is not merely an epoch-making book; it is revolutionary. It discards phlogiston altogether, alleging that the elements really concerned in combustion are oxygen and heat. It claims

that acids are compounds of oxygen with a base, instead of mixtures of "earth" and water; that metals are simple elements, not compounds of "earth" and "phlogiston"; and that water itself, like air, is a compound of oxygen with another element.

In applying these ideas the new system proposes an altogether new nomenclature for chemical substances. Hitherto the terminology of the science has been a matter of whim and caprice. Such names as "liver of sulfur," "mercury of life," "horned moon," "the double secret," "the salt of many virtues," and the like, have been accepted without protest by the chemical world. With such a terminology continued progress was as impossible as human progress without speech. The new chemistry of Lavoisier and his confrères, following the model set by zoology half a century earlier, designates each substance by a name instead of a phrase, applies these names according to fixed rules, and, in short, classifies the chemical knowledge of the time and brings it into a system, lacking which no body of knowledge has full title to the name of science.

Tho Lavoisier was not alone in developing this revolutionary scheme, posterity remembers him as its originator. His dazzling and comprehensive genius obscured the feebler lights of his confrères. Perhaps, too, his tragic fate was not without influence, in augmenting his posthumous fame. In 1794 he fell by the guillotine, guiltless of any crime but patriotism—a victim of the "Reign of Terror." "The Republic has no need of savants," remarked the functionary who signed the death-warrant of the most famous chemist of the century.

The leader of the reform movement in chemistry thus died at the hands of bigotry and fanaticism—rather, let

us say, as the victim of a national frenzy—while the cause he championed was young, yet not too soon to see the victory as good as won. The main body of French chemists had accepted the new doctrines almost from the first, and elsewhere the opposition had been of that fierce, eager type which soon exhausts itself in the effort.

At Berlin they began by burning Lavoisier in effigy, but they ended speedily by accepting the new theories. In England the fight was more stubborn, but equally decisive. At first the new chemistry was opposed by such great men as Black, of "latent heat" fame; Rutherford, the discoverer of nitrogen; and Cavendish, the inventor of the pneumatic trough and the discoverer of the composition of water, not to mention a coterie of lesser lights; but one by one they wavered and went over to the enemy.

Oddly enough, the doughtiest and most uncompromising of all the champions of the old "phlogistic" ideas was Dr. Priestley, the very man whose discovery of oxygen had paved the way for the "antiphlogistic" movement—a fact which gave rise to Cuvier's remark that Priestley was undoubtedly one of the fathers of modern chemistry, but a father who never wished to recognize his daughter.

A most extraordinary man was this Dr. Priestley. Davy said of him, a generation later, that no other person ever discovered so many new and curious substances as he; yet to the last he was only an amateur in science, his profession being the ministry. There is hardly another case in history of a man not a specialist in science accomplishing so much in original research as did Joseph Priestley, the chemist, physiologist, electrician; the mathematician, logician, and moralist; the theologian,



ALESSANDRO VOLTA

mental philosopher, and political economist. He took all knowledge for his field; but how he found time for his numberless researches and multifarious writings, along with his every-day duties, must ever remain a mystery to ordinary mortals.

That this marvelously receptive, flexible mind should have refused acceptance to the clearly logical doctrines of the new chemistry seems equally inexplicable. But so it was. To the very last, after all his friends had capitulated, Priestley kept up the fight. From America, whither he had gone to live in 1794, he sent out the last defiance to the enemy in 1800, in a brochure entitled *The Doctrine of Phlogiston Upheld*. In the mind of its author this was little less than a pæan of victory; but all the world besides knew that it was the swan-song of the doctrine of phlogiston. Despite the defiance of this single warrior the battle was really lost and won, and as the century closed, "antiphlogistic" chemistry had practical possession of the field.

Several causes conspired to make exploration all the fashion during the closing epoch of the eighteenth century. New aid to the navigator had been furnished by the perfected compass and quadrant, and by the invention of the chronometer; medical science had banished scurvy, which hitherto had been a perpetual menace to the voyager; and, above all, the restless spirit of the age impelled the venturesome to seek novelty in fields altogether new. Some started for the pole, others tried for a northeast or northwest passage to India, yet others sought the great antarctic continent told of by tradition. All these of course failed of their immediate purpose, but they added much to the world's store of knowledge and its fund of travelers' tales.

Among all these tales none was more remarkable

than those which told of strange living creatures found in antipodal lands. And here, as did not happen in every field, the narratives were often substantiated by the exhibition of specimens that admitted no question. Many a company of explorers returned more or less laden with such trophies from the animal and vegetable kingdoms, to the mingled astonishment, delight, and bewilderment of the closet naturalists. The followers of Linnæus in the "golden age of natural history," a few decades before, had increased the number of known species of fishes to about 400, of birds to 1000, of insects to 3000, and of plants to 10,000. But now these sudden accessions from new territories doubled the figure for plants, tripled it for fish and birds, and brought the number of described insects above 20,000.

Naturally enough, this wealth of new material was sorely puzzling to the classifiers. The more discerning began to see that the artificial system of Linnæus, wonderful and useful as it had been, must be advanced upon before the new material could be satisfactorily disposed of. The way to a more natural system, based on less arbitrary signs, had been pointed out by Jussieu in botany, but the zoologists were not prepared to make headway toward such a system until they should gain a wider understanding of the organisms with which they had to deal through comprehensive studies of anatomy. Such studies of individual forms in their relations to the entire scale of organic beings were pursued in these last decades of the century, but tho two or three most important generalizations were achieved (notably Kaspar Wolff's conception of the cell as the basis of organic life, and Goethe's doctrine of metamorphosis of parts), yet, as a whole, the work of the anatomists of the period was germinative rather than fruit-bearing.



Bichat's volumes, telling of the recognition of the fundamental tissues of the body, did not begin to appear till the last year of the century. The announcement by Cuvier of the doctrine of correlation of parts bears the same date, but in general the studies of this great naturalist, which in due time were to stamp him as the successor of Linnæus, were as yet only fairly begun.

In the field of physiology, on the other hand, two most important works were fairly consummated in this epoch—the longstanding problems of digestion and respiration were solved, almost coincidently. Two very distinguished physiologists share the main honors of discovery in regard to the function of digestion—the Abbé Spallanzani, of the University of Pavia, Italy, and John Hunter, of England. Working independently, these investigators showed at about the same time that digestion is primarily a chemical rather than a mechanical process. It is a curious commentary on the crude notions of mechanics of previous generations that it should have been necessary to prove by experiment that the thin, almost membranous stomach of a mammal has not the power to pulverize, by mere attrition, the foods that are taken into it. However, the proof was now for the first time forthcoming, and the question of the general character of the function of digestion was forever set at rest.

To clear up the mysteries of respiration was a task that fell to the lot of chemistry. The solution of the problem followed almost as a matter of course upon the advances of that science in the latter part of the century. Hitherto no one since Mayow, of the previous century, whose flash of insight had been strangely overlooked and forgotten, had even vaguely surmised the true function of the lungs. The great Boerhaave had supposed that respiration is chiefly important as an aid to the cir-

culation of the blood; his great pupil, Haller, had believed to the day of his death in 1777 that the main purpose of the function is to form the voice. No genius could hope to fathom the mystery of the lungs so long as air was supposed to be a simple element, serving a mere mechanical purpose in the economy of the earth.

But the discovery of oxygen gave the clew, and very soon all the chemists were testing the air that came from the lungs—Dr. Priestley, as usual, being in the van. His initial experiments were made in 1777, and from the outset the problem was as good as solved. Other experimenters confirmed his results in all their essentials—notably Scheele and Lavoisier and Spallanzani and Davy.

It was clearly established that there is chemical action in the contact of the air with the tissue of the lungs; that some of the oxygen of the air disappears, and that carbonic acid gas is added to the inspired air. It was shown, too, that the blood, having come in contact with the air, is changed from black to red in color. These essentials were not in dispute from the first. But just what chemical changes caused these results was the subject of controversy. Whether, for example, oxygen is actually absorbed into the blood, or whether it merely unites with carbon given off from the blood, was long in dispute.

Each of the main disputants was biased by his own particular views as to the moot points of chemistry. Lavoisier, for example, believed oxygen gas to be composed of a metal oxygen combined with the alleged element heat; Dr. Priestley thought it a compound of positive electricity and phlogiston; and Humphry Davy, when he entered the lists, a little later, supposed it to be a compound of oxygen and light. Such mistaken notions naturally complicated matters, and delayed a

complete understanding of the chemical processes of respiration.

It was some time, too, before the idea gained acceptance that the most important chemical changes do not occur in the lungs themselves, but in the ultimate tissues. Indeed, the matter was not clearly settled at the close of the century. Nevertheless, the problem of respiration had been solved in its essentials. Moreover, the vastly important fact had been established that a process essentially identical with respiration is necessary to the existence not only of all creatures supplied with lungs, but of fishes, insects, and even vegetables — in short, every kind of living organism.

All advances in science have a bearing, near or remote, on the welfare of our race; but we must credit to the closing decade of the eighteenth century a discovery which, in its power of direct and immediate benefit to humanity, surpasses any other discovery of this or any previous epoch. Needless to say I refer to Jenner's discovery of the method of preventing small-pox by inoculation with the virus of cow-pox.

It detracts nothing from the merit of this discovery to say that the preventive power of accidental inoculation had long been rumored among the peasantry of England. Such vague, unavailing half-knowledge is often the forerunner of fruitful discovery. To all intents and purposes Jenner's discovery was original and unique. Neither, considered as a perfected method, was it in any sense an accident.

It was a triumph of experimental science; how great a triumph it is difficult now to understand, for we of today can only vaguely realize what a ruthless and ever-present scourge small-pox had been to all previous generations of men since history began. Despite all efforts

to check it by medication and by direct inoculation, it swept now and then over the earth as a devastating pestilence, and year by year it claimed one-tenth of all the beings in Christendom by death as its average quota of victims. "From small-pox and love but few remain free," ran the old saw. A pitted face was almost as much a matter of course a hundred years ago as a smooth one is today.

Little wonder, then, that the world gave eager acceptance to Jenner's discovery. The first vaccination was made in 1796. Before the close of the century the method was practised everywhere in Christendom. No urging was needed to induce the majority to give it trial; passengers on a burning ship do not hold aloof from the life-boats. Rich and poor, high and low, sought succor in vaccination, and blessed the name of their deliverer. Of all the great names that were before the world in the closing days of the century, there was perhaps no other at once so widely known and so uniformly revered as that of the English physician Edward Jenner. Surely there was no other one that should be recalled with greater gratitude by posterity.



VOLTA AND NAPOLEON

49

Ich gehe in meine Zimmern, habe noch einen
Blick auf den Hof und den Garten, und dann
zu Bett. Ich habe noch einen Blick auf den
Hof und den Garten, und dann zu Bett.
Ich habe noch einen Blick auf den Hof und den
Garten, und dann zu Bett.

Mit vorzüglicher Gefügung

Shirley

Vol. 42

1815

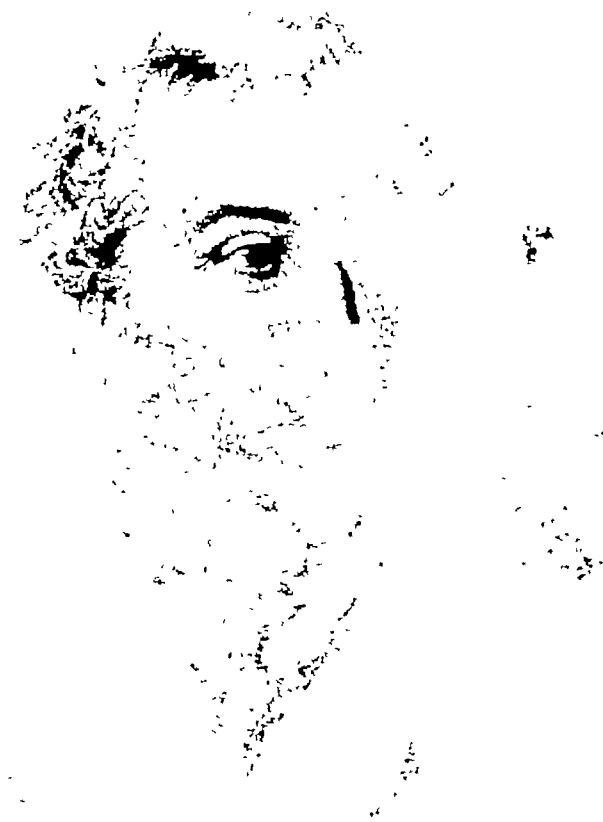


JOHANN WOLFGANG VON GOETHE.

Date: 11/15

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THOMAS YOUNG

II

A CENTURY'S PROGRESS IN PHYSICS

THERE were giants abroad in the world of science in the early days of the nineteenth century. Herschel, Lagrange, and Laplace; Cuvier, Brongniart, and Lamarck; Humboldt, Goethe, Priestley—what need to extend the list?—the names crowd upon us. But among them all there was no taller intellectual figure than that of a young Quaker who came to settle in London and practise the profession of medicine in the year 1801.

The name of this young aspirant to medical honors and emoluments was Thomas Young. He came fresh from professional studies at Edinburgh and on the Continent, and he had the theory of medicine at his tongue's end; yet his medical knowledge, compared with the mental treasures of his capacious intellect as a whole, was but as a drop of water in the ocean.

For it chanced that this young Quaker physician was one of those prodigies who come but few times in a century, and the full list of whom in the records of history could be told on one's thumbs and fingers. His biographers tell us things about him that read like the most patent fairy-tales. As a mere infant in arms he had been able to read fluently. Before his fourth birthday came he had read the Bible twice through, as well as Watts's *Hymns*—poor child!—and when seven or eight he had shown a propensity to absorb languages much as other children absorb nursery tattle and Mother Goose rimes. When he was fourteen, a young lady visiting the

household of his tutor patronized the pretty boy by asking to see a specimen of his penmanship. The pretty boy complied readily enough, and mildly rebuked his interrogator by rapidly writing some sentences for her in fourteen languages, including such as Arabian, Persian, and Ethiopic.

Meantime languages had been but an incident in the education of the lad. He seems to have entered every available field of thought—mathematics, physics, botany, literature, music, painting, languages, philosophy, archeology, and so on to tiresome lengths—and once he had entered any field he seldom turned aside until he had reached the confines of the subject as then known, and added something new from the recesses of his own genius. He was as versatile as Priestley, as profound as Newton himself. He had the range of a mere dilettante, but everywhere the full grasp of the master. He took early for his motto the saying that what one man has done, another man may do. Granting that the other man has the brain of a Thomas Young, it is a true motto.

Such then was the young Quaker who came to London to follow out the humdrum life of a practitioner of medicine in the year 1801. But incidentally the young physician was prevailed upon to occupy the interims of early practise by fulfilling the duties of the chair of Natural Philosophy at the Royal Institution, which Count Rumford had founded, and of which Davy was then Professor of Chemistry—the institution whose glories have been perpetuated by such names as Faraday and Tyndall, and which the Briton of today speaks of as the "Pantheon of Science." Here it was that Thomas Young made those studies which have insured him a niche in the temple of fame not far removed from that of Isaac Newton.

As early as 1793, when he was only twenty, Young had begun to communicate papers to the Royal Society of London, which were adjudged worthy to be printed in full in the *Philosophical Transactions*; so it is not strange that he should have been asked to deliver the Bakerian lecture before that learned body the very first year after he came to London. The lecture was delivered November 12, 1801. Its subject was "The Theory of Light and Colors," and its reading marks an epoch in physical science; for here was brought forward for the first time convincing proof of that undulatory theory of light with which every student of modern physics is familiar—the theory which holds that light is not a corporeal entity, but a mere pulsation in the substance of an all-pervading ether, just as sound is a pulsation in the air, or in liquids or solids.

Young had, indeed, advocated this theory at an earlier date, but it was not until 1801 that he hit upon the idea which enabled him to bring it to anything approaching a demonstration. It was while pondering over the familiar but puzzling phenomena of colored rings into which white light is broken when reflected from thin films—Newton's rings, so called—that an explanation occurred to him which at once put the entire undulatory theory on a new footing.

With that sagacity of insight which we call genius, he saw of a sudden that the phenomena could be explained by supposing that when rays of light fall on a thin glass, part of the rays being reflected from the upper surface, other rays, reflected from the lower surface, might be so retarded in their course through the glass that the two sets would interfere with one another, the forward pulsation of one ray corresponding to the backward pulsation of another, thus quite neutralizing

the effect. Some of the component pulsations of the light being thus effaced by mutual interference, the remaining rays would no longer give the optical effect of white light; hence the puzzling colors.

By following up this clue with mathematical precision, measuring the exact thickness of the plate and the space between the different rings of color, Young was able to show mathematically what must be the length of pulsation for each of the different colors of the spectrum. He estimated that the undulations of red light, at the extreme lower end of the visible spectrum, must number about 37,640 to the inch, and pass any given spot at a rate of 463 millions of millions of undulations in a second, while the extreme violet numbers 59,750 undulations to the inch, or 735 millions of millions to the second.

Young similarly examined the colors that are produced by scratches on a smooth surface, in particular testing the light from "Mr. Coventry's exquisite micrometers," which consist of lines scratched on glass at measured intervals. These microscopic tests brought the same results as the other experiments. The colors were produced at certain definite and measurable angles, and the theory of interference of undulations explained them perfectly, while, as Young affirmed with confidence, no other theory hitherto advanced could explain them at all. Taking all the evidence together, Young declared that he considered the argument he had set forth in favor of the undulatory theory of light to be "sufficient and decisive."

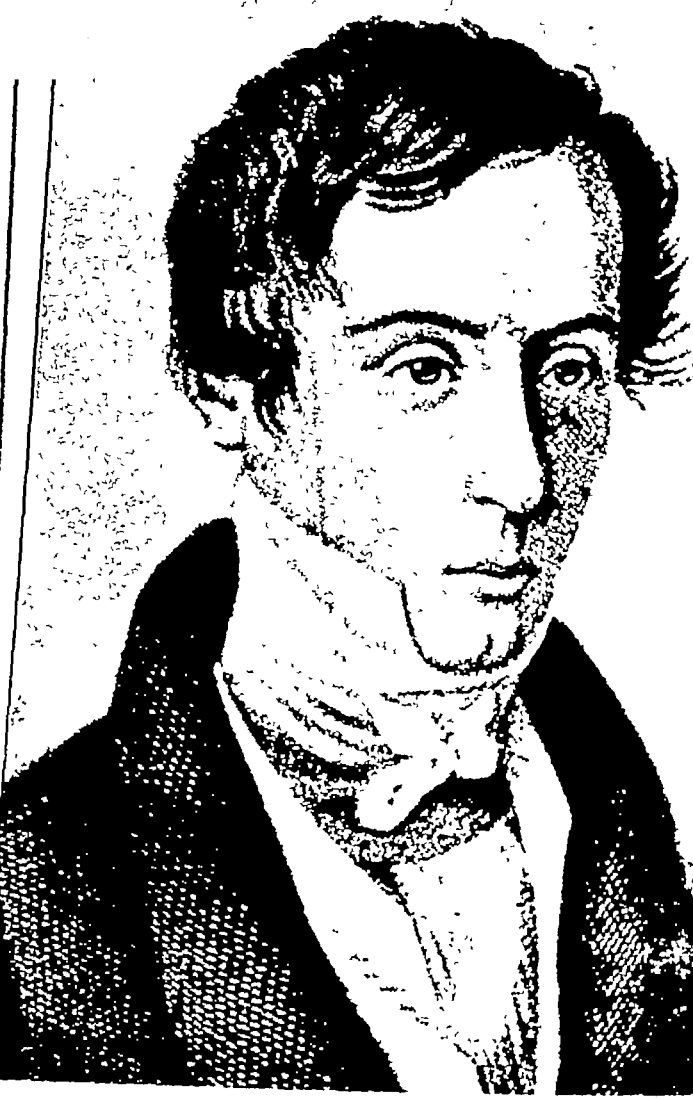
This doctrine of interference of undulations was the absolutely novel part of Young's theory. The all-compassing genius of Robert Hooke had, indeed, very nearly apprehended it more than a century before, as Young

himself points out, but no one else had so much as vaguely conceived it; and even with the sagacious Hooke it was only a happy guess, never distinctly outlined in his own mind, and utterly ignored by all others. Young did not know of Hooke's guess until he himself had fully formulated the theory, but he hastened then to give his predecessor all the credit that could possibly be adjudged his due by the most disinterested observer.

To Hooke's contemporary, Huygens, who was the originator of the general doctrine of undulation as the explanation of light, Young renders full justice also. For himself he claims only the merit of having demonstrated the theory which these and a few others of his predecessors had advocated without full proof.

The following year Dr. Young detailed before the Royal Society other experiments, which threw additional light on the doctrine of interference; and in 1803 he cited still others, which, he affirmed, brought the doctrine to complete demonstration. In applying this demonstration to the general theory of light, he made the striking suggestion that "the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely, perhaps, as the wind passes through a grove of trees." He asserted his belief also that the chemical rays which Ritter had discovered beyond the violet end of the visible spectrum are but still more rapid undulations of the same character as those which produce light.

In his earlier lecture he had affirmed a like affinity between the light rays and the rays of radiant heat which Herschel detected below the red end of the spectrum, suggesting that "light differs from heat only in the frequency of its undulations or vibrations—those undulations which are within certain limits with respect to



AUGUSTIN FRESNEL

frequency affecting the optic nerve and constituting light, and those which are slower and probably stronger constituting heat only." From the very outset he had recognized the affinity between sound and light; indeed, it had been this affinity that led him on to an appreciation of the undulatory theory of light.

But while all these affinities seemed so clear to the great coordinating brain of Young, they made no such impression on the minds of his contemporaries. The immateriality of light had been substantially demonstrated, but practically no one save its author accepted the demonstration. Newton's doctrine of the emission of corpuscles was too firmly rooted to be readily dislodged, and Dr. Young had too many other interests to continue the assault unceasingly.

He occasionally wrote something touching on his theory, mostly papers contributed to the *Quarterly Review* and similar periodicals, anonymously or under a pseudonym, for he had conceived the notion that too great conspicuousness in fields outside of medicine would injure his practise as a physician. His views regarding light (including the original papers from the *Philosophical Transactions of the Royal Society*) were again given publicity in full in his celebrated volume on natural philosophy, consisting in part of his lectures before the Royal Institution, published in 1807; but even then they failed to bring conviction to the philosophic world. Indeed, they did not even arouse a controversial spirit, as his first papers had done.

So it chanced that when, in 1815, a young French military engineer, Augustin Jean Fresnel, returning from the Napoleonic wars, became interested in the phenomena of light, and made some experiments concerning diffraction, which seemed to him to controvert the ac-

cepted notions of the materiality of light, he was quite unaware that his experiments had been anticipated by a philosopher across the Channel. He communicated his experiments and results to the French Institute, supposing them to be absolutely novel. That body referred them to a committee, of which, as good fortune would have it, the dominating member was Dominique François Arago, a man as versatile as Young himself, and hardly less profound, if perhaps not quite so original.

Arago at once recognized the merit of Fresnel's work, and soon became a convert to the theory. He told Fresnel that Young had anticipated him as regards the general theory, but that much remained to be done, and he offered to associate himself with Fresnel in prosecuting the investigation. Fresnel was not a little dashed to learn that his original ideas had been worked out by another while he was a lad, but he bowed gracefully to the situation, and went ahead with unabated zeal.

The championship of Arago insured the undulatory theory a hearing before the French Institute, but by no means sufficed to bring about its general acceptance. On the contrary, a bitter feud ensued, in which Arago was opposed by the "Jupiter Olympius of the Academy," Laplace, by the only less famous Poisson, and by the younger but hardly less able Biot. So bitterly raged the feud that a lifelong friendship between Arago and Biot was ruptured forever. The opposition managed to delay the publication of Fresnel's papers, but Arago continued to fight with his customary enthusiasm and pertinacity, and at last, in 1823, the Academy yielded and voted Fresnel into its ranks, thus implicitly admitting the value of his work.

It is a humiliating thought that such controversies as this must mar the progress of scientific truth; but for-

unately the story of the introduction of the undulatory theory has a more pleasant side. Three men, great both in character and in intellect, were concerned in pressing its claims—Young, Fresnel and Arago—and the relations of these men form a picture unmarred by any of those petty jealousies that so often dim the luster of great names. Fresnel freely acknowledged Young's priority so soon as his attention was called to it; and Young applauded the work of the Frenchman, and aided with his counsel in the application of the undulatory theory to the problems of polarization of light, which still demanded explanation, and which Fresnel's fertility of experimental resource and profundity of mathematical insight sufficed in the end to conquer.

After Fresnel's admission to the Institute in 1823 the opposition weakened, and gradually the philosophers came to realize the merits of a theory which Young had vainly called to their attention a full quarter-century before. Now, thanks largely to Arago, both Young and Fresnel received their full meed of appreciation.

Fresnel was given the Rumford medal of the Royal Society of England in 1825, and chosen one of the foreign members of the Society two years later, while Young in turn was elected one of the eight foreign members of the French Academy.

As a fitting culmination of the chapter of felicities between the three friends, it fell to the lot of Young, as Foreign Secretary of the Royal Society, to notify Fresnel of the honors shown him by England's representative body of scientists; while Arago, as Perpetual Secretary of the French Institute, conveyed to Young in the same year the notification that he had been similarly honored by the savants of France.

A few months later Fresnel was dead, and Young

survived him only two years. Both died prematurely; but their great work was done, and the world will remember always and link together these two names in connection with a theory which in its implications and importance ranks little below the theory of universal gravitation.

The full importance of Young's studies of light might perhaps have gained earlier recognition had it not chanced that, at the time when they were made, the attention of the philosophic world was turned with the fixity and fascination of a hypnotic stare upon another field, which for a time brooked no rival. How could the old familiar phenomenon light interest any one when the new agent galvanism was in view? As well ask one to fix attention on a star while a meteorite blazes across the sky.

The question of the hour was whether in galvanism the world had to do with a new force, or whether it is identical with electricity, masking under a new form. Very early in the century the profound, if rather capacious, Dr. Wollaston made experiments which seemed to show that the two are identical; and by 1807 Dr. Young could write in his published lectures: "The identity of the general causes of electrical and of galvanic effects is now doubted by few." To be entirely accurate, he should have added, "by few of the leaders of scientific thought," for the lesser lights were by no means so fully agreed as the sentence cited might seem to imply.

But meantime an even more striking affinity had been found for the new agent galvanism. From the first it had been the chemists rather than the natural philosophers—the word physicist was not then in vogue—who had chiefly experimented with Volta's battery; and the acute mind of Humphry Davy at once recognized the

close relationship between chemical decomposition and the appearance of the new "imponderable." The great Swedish chemist Berzelius also had an inkling of the same thing. But it was Davy who first gave the thought full expression, in a Bakerian lecture before the Royal Society in 1806—the lecture which gained him not only the plaudits of his own countrymen, but the Napoleonic prize of the French Academy at a time when the political bodies of the two countries were in the midst of a sanguinary war.

"Science knows no country," said the young Englishman, in accepting the French testimonial, against the wishes of some of the more narrow-minded of his friends. "If the two countries or governments are at war, the men of science are not. That would, indeed, be a civil war of the worst description. We should rather, through the instrumentality of men of science, soften the asperities of national hostility."

Here it was that Davy explicitly stated his belief that "chemical and electrical attraction are produced by the same cause, acting in one case on particles, in the other on masses," and that "the same property, under different modifications, is the cause of all the phenomena exhibited by different voltaic combinations." The phenomena of galvanism were thus linked with chemical action on the one hand, and with frictional electricity on the other, in the first decade of the century, showing that electricity is by no means the isolated "fluid" that it had been thought. But there the matter rested for another decade.

The imaginative Davy, whose penetrative genius must have carried him further had it not been diverted, became more and more absorbed in the chemical side of the problem; and Young, having severed his connection



HANS CHRISTIAN OERSTED

with the Royal Institution, was devoting himself to developing his medical practise, and in intervals of duty to deciphering Egyptian hieroglyphics. Parenthetically it may be added that Young was far too much in advance of his time to make a great success as a practitioner (people demand sophistry rather than philosophy of their family physician), but that his success with the hieroglyphics was no less novel and epoch-making than his work in philosophy.

For a time no master-generalizer came to take the place of these men in the study of the "imponderables" as such, and the phenomena of electricity occupied an isolated corner in the realm of science, linked, as has been said, rather to chemistry than to the field we now term physics.

But in 1819 there flashed before the philosophic world, like lightning from a clear sky, the report that Hans Christian Oersted, the Danish philosopher, had discovered that the magnetic needle may be deflected by the passage near it of a current of electricity. The experiment was repeated everywhere. Its validity was beyond question, its importance beyond estimate. Many men had vaguely dreamed that there might be some connection between electricity and magnetism—chiefly because each shows phenomena of seeming attraction and repulsion—but here was the first experimental evidence that any such connection actually exists. The wandering eye of science was recalled to electricity as suddenly and as irresistibly as it had been in 1800 by the discovery of the voltaic pile. But now it was the physical rather than the chemical side of the subject that chiefly demanded attention.

At once André Marie Ampère, whom the French love to call the Newton of electricity, appreciated the far-

reaching importance of the newly disclosed relationship, and, combining mathematical and experimental studies, showed how close is the link between electricity and magnetism, and suggested the possibility of signaling at a distance by means of electric wires associated with magnetic needles. Gauss, the great mathematician, and Weber, the physicist, put this idea to a practical test by communicating with one another at a distance of several rods, in Göttingen, long before "practical" telegraphy grew out of Oersted's discovery.

A new impetus thus being given to the investigators, an epoch of electrical discovery naturally followed. For a time interest centered on the French investigators, in particular upon the experiments of the ever-receptive Arago, who discovered in 1825 that magnets may be produced at will by electrical induction. But about 1830 the scene shifted to London; for then the protégé of Davy, and his successor in the Royal Institution, Michael Faraday, the "man who added to the powers of his intellect all the graces of the human heart," began that series of electrical experiments at the Royal Institution which were destined to attract the dazed attention of the philosophic world, and stamp their originator as "the greatest experimental philosopher the world has ever seen."

Nor does the rank of prince of experimenters do Faraday full justice, for he was far more than a mere experimenter. He had not, perhaps, quite the intuitive insight of Davy, and he utterly lacked the profound mathematical training of Young. None the less was he a man who could dream dreams on occasion, and, as Maxwell has insisted, think in mathematical channels if not with technical symbols. Only his wagon must always traverse earth tho hitched to a star. His dreams



guided him onward, but ever the hand of experiment kept check over the dreams.

It was in 1831 that Faraday opened up the field of magneto-electricity. Reversing the experiments of his predecessors, who had found that electric currents may generate magnetism, he showed that magnets have power under certain circumstances to generate electricity; he proved, indeed, the interconvertibility of electricity and magnetism. Then he showed that all bodies are more or less subject to the influence of magnetism, and that even light may be affected by magnetism as to its phenomena of polarization. He satisfied himself completely of the true identity of all the various forms of electricity, and of the convertibility of electricity and chemical action. Thus he linked together light, chemical affinity, magnetism, and electricity. And, moreover, he knew full well that no one of these can be produced in indefinite supply from another. "Nowhere," he says, "is there a pure creation or production of power without a corresponding exhaustion of something to supply it."

When Faraday wrote those words in 1840 he was treading on the very heels of a greater generalization than any which he actually formulated; nay, he had it fairly within his reach. He saw a great truth without fully realizing its import; it was left for others, approaching the same truth along another path, to point out its full significance.

The great generalization which Faraday so narrowly missed, and which later became familiar as the doctrine of the conservation of energy—is the law that in transforming energy from one condition to another we can never secure more than an equivalent quantity; that, in short, "to create or annihilate energy is as impossible



MICHAEL FARADAY



FARADAY'S LABORATORY

as to create or annihilate matter; and that all the phenomena of the material universe consist in transformations of energy alone."

Some philosophers have pronounced this the greatest generalization ever conceived by the mind of man. Be that as it may, it is surely one of the great intellectual landmarks of the nineteenth century.

A vast generalization such as this is never a mushroom growth, nor does it usually spring full-grown from the mind of any single man. Always a number of minds are very near a truth before any one mind fully grasps it. Preeminently true is this of the doctrine of conservation of energy. Not Faraday alone, but half a dozen different men had an inkling of it before it gained full expression; indeed, every man who advocated the undulatory theory of light and heat was verging toward the goal.

The doctrine of Young and Fresnel was as a highway leading surely on to the wide plain of conservation. The phenomena of electro-magnetism furnished another such highway. But there was yet another road which led just as surely and even more readily to the same goal. This was the road furnished by the phenomena of heat, and the men who traveled it were destined to outstrip their fellow-workers; tho, as we have seen, wayfarers on other roads were within hailing distance when the leaders passed the mark.

In order to do even approximate justice to the men who entered into the great achievement, we must recall that just at the close of the last century Count Rumford and Humphry Davy independently showed that labor may be transformed into heat; and correctly interpreted this fact as meaning the transformation of molar into molecular motion. We can hardly doubt that each of

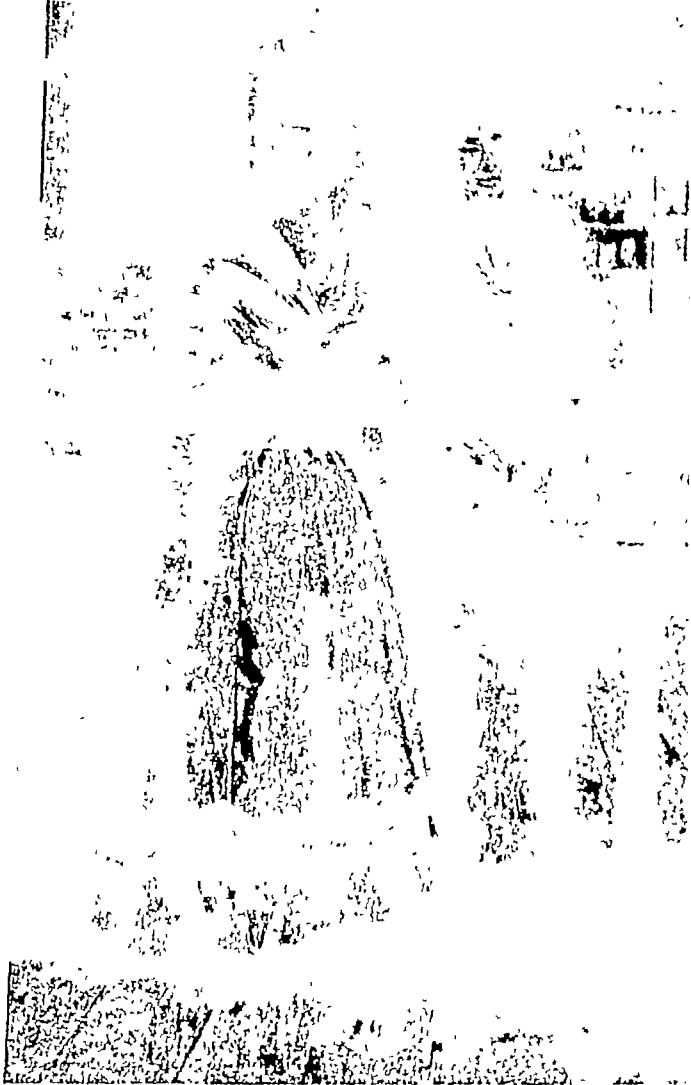


• JAMES PRESCOTT JOULE

these men of genius realized, vaguely, at any rate, that there must be a close correspondence between the amount of the molar and the molecular motions; hence that each of them was in sight of the law of the mechanical equivalent of heat. But neither of them quite grasped or explicitly stated what each must vaguely have seen; and for just a quarter of a century no one else even came abreast their line of thought, let alone passing it.

But then, in 1824, a French philosopher, Sadi Carnot, caught step with the great Englishmen, and took a long leap ahead by explicitly stating his belief that a definite quantity of work could be transformed into a definite quantity of heat, no more, no less. Carnot did not, indeed, reach the clear view of his predecessors as to the nature of heat, for he still thought it a form of "imponderable" fluid; but he reasoned none the less clearly as to its mutual convertibility with mechanical work. *But important as his conclusions seem now that we look back upon them with clearer vision, they made no impression whatever upon his contemporaries.* Carnot's work in this line was an isolated phenomenon of historical interest, but it did not enter into the scheme of the completed narrative in any such way as did the work of Rumford and Davy.

The man who really took up the broken thread where Rumford and Davy had dropped it, and wove it into a completed texture, came upon the scene in 1840. His home was in Manchester, England; his occupation that of a manufacturer. He was a friend and pupil of the great Dr. Dalton. His name was James Prescott Joule. When posterity has done its final juggling with the names of the nineteenth century, it is not unlikely that the name of this Manchester philosopher will be a



FARADAY AND HIS WIFE

household word like the names of Aristotle, Copernicus, and Newton.

For Joule's work it was, done in the fifth decade of that century, which demonstrated beyond all cavil that there is a precise and absolute equivalence between mechanical work and heat; that whatever the form of manifestation of molar motion, it can generate a definite and measurable amount of heat, and no more.

Joule found, for example, that at the sea-level in Manchester a pound weight falling through 772 feet could generate enough heat to raise the temperature of a pound of water one degree Fahrenheit. There was nothing haphazard, nothing accidental, about this; it bore the stamp of unalterable law. And Joule himself saw, what others in time were made to see, that this truth is merely a particular case within a more general law. If heat cannot be in any sense created, but only made manifest as a transformation of another kind of motion, then must not the same thing be true of all those other forms of "force"—light, electricity, magnetism—which had been shown to be so closely associated, so mutually convertible, with heat? All analogy seemed to urge the truth of this inference; all experiment tended to confirm it.

The law of the mechanical equivalent of heat then became the main corner-stone of the greater law of the conservation of energy.

Even as Joule experimented with the transformation of heat, a philosopher of Copenhagen, Colding by name, had hit upon the same idea, and carried it far toward a demonstration. And then, without pausing, we must shift yet again, this time to Germany, and consider the work of three other men, who independently were on the track of the same truth, and two of whom, it must

be admitted, reached it earlier than either Joule or Colding, if neither brought it to quite so clear a demonstration. The names of these three Germans are Mohr, Mayer, and Helmholtz. Their share in establishing the great doctrine of conservation must now claim our attention.

As to Karl Friedrich Mohr, it may be said that his statement of the doctrine preceded that of any of his fellows, yet that otherwise it was perhaps least important. In 1837 this thoughtful German had grasped the main truth, and given it expression in an article published in the *Zeitschrift für Physik*. But the article attracted no attention whatever, even from Mohr's own countrymen. Still, Mohr's title to rank as one who independently conceived the great truth, and perhaps first conceived it before any other man in the world saw it as clearly, even tho he did not demonstrate its validity, is not to be disputed.

It was just five years later, in 1842, that Dr. Julius Robert Mayer, practising physician in the little German town of Heilbronn, published a paper in Liebig's *Annalen* on "The Forces of Inorganic Nature," in which not merely the mechanical theory of heat, but the entire doctrine of the conservation of energy, is explicitly if briefly stated. Two years earlier Dr. Mayer, while surgeon to a Dutch India vessel cruising in the tropics, had observed that the venous blood of a patient seemed redder than venous blood usually is observed to be in temperate climates. He pondered over this seemingly insignificant fact, and at last reached the conclusion that the cause must be the lesser amount of oxidation required to keep up the body temperature in the tropics.

Led by this reflection to consider the body as a machine dependent on outside forces for its capacity to



Ch. 100

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ALEXANDER PUSHKIN

Nov. 11th. And 11th



act, he passed on into a novel realm of thought, which brought him at last to independent discovery of the mechanical theory of heat, and to the first full and comprehensive appreciation of the great law of conservation. Blood-letting, the modern physician holds, was a practise of very doubtful benefit, as a rule, to the subject; but once, at least, it led to marvelous results. No straw is so small that it may not point the receptive mind of genius to new and wonderful truths.

Here, then, was this obscure German physician, leading the humdrum life of a village practitioner, yet seeing such visions as no human being in the world had ever seen before.

The great principle he had discovered became the dominating thought of his life, and filled all his leisure hours. He applied it far and wide, amidst all the phenomena of the inorganic and organic worlds. It taught him that both vegetables and animals are machines, bound by the same laws that hold sway over inorganic matter, transforming energy, but creating nothing. Then his mind reached out into space and met a universe made up of questions. Each star that blinked down at him as he rode in answer to a night call seemed an interrogation-point asking, How do I exist? Why have I not long since burned out if your theory of conservation be true? No one hitherto had even tried to answer that question; few had so much as realized that it demanded an answer. But the Heilbronn physician understood the question and found an answer. His meteoric hypothesis, published in 1848, gave for the first time a tenable explanation of the persistent light and heat of our sun and the myriad other suns—an explanation to which we shall recur in another connection.

All this time our isolated philosopher, his brain



HERMANN L. F. VON HELMHOLTZ

afire with the glow of creative thought, was quite unaware that any one else in the world was working along the same lines. And the outside world was equally heedless of the work of the Heilbronn physician. There was no friend to inspire enthusiasm and give courage, no kindred spirit to react on this masterful but lonely mind. And this is the more remarkable because there are few other cases where a master-originator in science has come upon the scene except as the pupil or friend of some other master-originator.

Of the men we have noticed in the present connection, Young was the friend and confrère of Davy; Davy, the protégé of Rumford; Faraday, the pupil of Davy; Fresnel, the co-worker with Arago; Colding, the confrère of Oersted; Joule, the pupil of Dalton. But Mayer is an isolated phenomenon—one of the lone mountain-peak intellects of the century. That estimate may be exaggerated which has called him the Galileo of the nineteenth century, but surely no lukewarm praise can do him justice.

Yet for a long time his work attracted no attention whatever. In 1847, when another German physician, Hermann von Helmholtz, one of the most massive and towering intellects of any age, had been independently led to comprehension of the doctrine of conservation of energy, and published his treatise on the subject, he had hardly heard of his countryman Mayer. When he did hear of him, however, he hastened to renounce all claim to the doctrine of conservation, tho the world at large gives him credit of independent even tho subsequent discovery.

Meantime in England Joule was going on from one experimental demonstration to another, oblivious of his German competitors and almost as little noticed by his

own countrymen. He read his first paper before the chemical section of the British Association for the Advancement of Science in 1843, and no one heeded it in the least. Two years later he wished to read another paper, but the chairman hinted that time was limited, and asked him to confine himself to a brief verbal synopsis of the results of his experiments. Had the chairman but known it, he was curtailing a paper vastly more important than all the other papers of the meeting put together. However, the synopsis was given, and one man was there to hear it who had the genius to appreciate its importance.

This was William Thomson, later Lord Kelvin, now remembered as among the greatest of natural philosophers, but then only a novitiate in science. He came to Joule's aid, started rolling the ball of controversy, and subsequently associated himself with the Manchester experimenter in pursuing his investigations.

But meantime the acknowledged leaders of British science viewed the new doctrine askance. Faraday, Brewster, Herschel—those were the great names in physics at that day, and no one of them could quite accept the new views regarding energy. For several years no older physicist, speaking with recognized authority, came forward in support of the doctrine of conservation.

This culminating thought of the half-century came silently into the world, unheralded and unopposed. The fifth decade of the century had seen it elaborated and substantially demonstrated in at least three different countries, yet even the leaders of thought did not so much as know of its existence. In 1853 Whewell, the historian of the inductive sciences, published a second edition of his history, and, as Huxley has point-



ed out, he did not so much as refer to the revolutionizing thought which even then was a full decade old.

By this time, however, the battle was brewing. The rising generation saw the importance of a law which their elders could not appreciate, and soon it was noised abroad that there were more than one claimant to the honor of discovery. Chiefly through the efforts of Professor Tyndall, the work of Mayer became known to the British public, and a most regrettable controversy ensued between the partizans of Mayer and those of Joule—a bitter controversy, in which Davy's contention that science knows no country was not always regarded, and which left its scars upon the hearts and minds of the great men whose personal interests were involved.

And so to this day the question who is the chief discoverer of the law of conservation of energy is not susceptible of a categorical answer that would satisfy all philosophers. It is generally held that the first choice lies between Joule and Mayer. Professor Tyndall has expressed the belief that in future each of these men will be equally remembered in connection with this work. But history gives us no warrant for such a hope. Posterity in the long-run demands always that its heroes shall stand alone. Who remembers now that Robert Hooke contested with Newton the discovery of the doctrine of universal gravitation?

The judgment of posterity is unjust, but it is inexorable. And so we can little doubt that a century from now one name will be mentioned as that of the originator of the great doctrine of conservation of energy. The man whose name is thus remembered will perhaps be spoken of as the Galileo, the Newton, of the nineteenth century; but whether the name thus dignified by

the final verdict of history will be that of Colding, Mohr, Mayer, Helmholtz, or Joule, it is still too early to decide.

The gradual permeation of the field by the great doctrine of conservation simply repeated the history of the introduction of every novel and revolutionary thought. Necessarily the elder generation, to whom all forms of energy were imponderable fluids, must pass away before the new conception could claim the field. Even the word energy, tho Young had introduced it in 1807, did not come into general use till some time after the middle of the century. To the generality of philosophers (the word physicist was even less in favor at this time) the various forms of energy were still subtle fluids, and never was idea relinquished with greater unwillingness than this. The experiments of Young and Fresnel had convinced a large number of philosophers that light is a vibration and not a substance; but so great an authority as Biot clung to the old emission idea to the end of his life, in 1862, and held a following.

Meantime, however, the company of brilliant young men who had just served their apprenticeship when the doctrine of conservation came upon the scene had grown into authoritative positions, and were battling actively for the new ideas. Confirmatory evidence that energy is a molecular motion and not an "imponderable" form of matter accumulated day by day. The experiments of two Frenchmen, Hippolyte L. Fizeau and Léon Foucault, served finally to convince the last lingering skeptics that light is an undulation; and by implication brought heat into the same category, since James David Forbes, the Scotch physicist, had shown in 1837 that radiant heat conforms to the same laws of polarization and double refraction that govern light.

But, for that matter, the experiments that had established the mechanical equivalent of heat hardly left room for doubt as to the immateriality of this "imponderable."

Doubters had, indeed, expressed skepticism as to the validity of Joule's experiments, but the further researches, experimental and mathematical, of such workers as Thomson (Lord Kelvin), Rankine, and Tyndall in Great Britain, of Helmholtz and Clausius in Germany, and of Regnault in France, dealing with various manifestations of heat, placed the evidence beyond the reach of criticism.

Out of these studies, just at the middle of the century, to which the experiments of Mayer and Joule had led, grew the new science of thermodynamics. Out of them also grew in the mind of one of the investigators a new generalization, only second in importance to the doctrine of conservation itself. Professor William Thomson (Lord Kelvin) in his studies in thermodynamics was early impressed with the fact that whereas all the molar motion developed through labor or gravity could be converted into heat, the process is not fully reversible. Heat can, indeed, be converted into molar motion or work, but in the process a certain amount of the heat is radiated into space and lost. Indeed, every transmutation of energy, of whatever character, seems complicated by a tendency to develop heat, part of which is lost.

This observation led Professor Thomson to his doctrine of the dissipation of energy, which he formulated before the Royal Society of Edinburgh in 1852, and published also in the *Philosophical Magazine* the same year, the title being, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy."



JAMES CLERK MAXWELL

From the principle here expressed Professor Thomson drew the startling conclusion that, "since any restoration of this mechanical energy without more than an equivalent dissipation is impossible," the universe, as known to us, must be in the condition of a machine gradually running down; and in particular that the world we live on has been within a finite time unfit for human habitation, and must again become so within a finite future.

This thought seems such a commonplace today that it is difficult to realize how startling it appeared nearly a century ago. A generation trained in the doctrines of conservation and dissipation of energy as the very alphabet of physical science can but ill appreciate the mental attitude of a generation which for the most part had not even thought it problematical whether the sun could continue to give out heat and light forever. But those advance thinkers who had grasped the import of the doctrine of conservation could at once appreciate the force of Thomson's doctrine of dissipation, and realize the complementary character of the two conceptions.

Here and there a thinker like Rankine did, indeed, attempt to fancy conditions under which the energy lost through dissipation might be restored to availability, but no such effort has met with success, and in time Professor Thomson's generalization and his conclusions as to the consequences of the law involved came to be universally accepted.

The introduction of the new views regarding the nature of energy followed, as I have said, the course of every other growth of new ideas. Young and imaginative men could accept the new point of view; older philosophers, their minds channeled by preconceptions,

could not get into the new groove. So strikingly true is this in the particular case now before us that it is worth while to note the ages at the time of the revolutionary experiments of the men whose work has been mentioned as entering into the scheme of evolution of the idea that energy is merely a manifestation of matter *in motion*. Such a list will tell the story better than a volume of commentary.

Observe, then, that Davy made his epochal experiment of melting ice by friction when he was a youth of twenty. Young was no older when he made his first communication to the Royal Society, and was in his twenty-seventh year when he first actively espoused the undulatory theory. Fresnel was twenty-six when he made his first important discoveries in the same field; and Arago, who at once became his champion, was then but two years his senior, tho for a decade he had been so famous that one involuntarily thinks of him as belonging to an elder generation.

Forbes was under thirty when he discovered the polarization of heat, which pointed the way to Mohr, then thirty-one, to the mechanical equivalent. Joule was twenty-two in 1840, when his great work was begun; and Mayer, whose discoveries date from the same year, was then twenty-six, which was also the age of Helmholtz when he published his independent discovery of the same law. William Thomson was a youth just past his majority when he came to the aid of Joule before the British Society, and but seven years older when he formulated his own doctrine of dissipation of energy. And Clausius and Rankine, who are usually mentioned with Thomson as the great developers of thermodynamics, were both far advanced with their novel studies before they were thirty. We may well agree with the

father of inductive science that "the man who is young in years may be old in hours."

Yet we must not forget that the shield has a reverse side. For was not the greatest of observing astronomers, Herschel, past thirty-five before he ever saw a telescope, and past fifty before he discovered the heat rays of the spectrum? And had not Faraday reached middle life before he turned his attention especially to electricity? Clearly, then, to make his phrase complete, Bacon must have added that "the man who is old in years may be young in imagination." Here, however, even more appropriate than in the other case—more's the pity—would have been the application of his qualifying clause: "but that happeneth rarely."



YOUNG JAMES WATT

The Eve of Saint Mark 1810



~~It was a fine holiday~~
~~Twice holy was the sabbath bell~~
 Upon a sabbath day it fell
 Twice holy was the sabbath bell;
 That call'd the folk to evening prayer—
 The city streets were clean and fair
 From wholesome death of April rains
 And on the western window panes
 The chilly sunset boded faintly told
 Of unarm'd green valleys cold
 Of the green thorny bloom left hedge
 Of rivers new with springtide sedge
 Of Pennines by shelter'd hills
 And daisies on the meadow hills
 Twice holy was the sabbath bell.
 The silent streets were crowded with
 Whole stand and pious companies
 From their fire side or stove
 And pacing morning with devout air
 To even song and vesper prayer

JOHN KEATS.

Date, 1818-19.

British Museum, Egerton MS 2780.



Handwritten: *Handwritten signature*

Bessie

je vous en remercie
 de la communication pour l'op-
 tante et une grande que Mrs
 Oindley bien me faire. je n'ai
 fait aucune modification à l'écrit
 biographique que Mrs Manning
 vous a plu et que Mrs Manning
 fait l'honneur de me l'envoyer.
 quelques points faits et indiqués
 sont moins graves à mon point de
 l'importance des appréciations.
 Or, je comprends que du apais
 tout dit on dit bien l'écrit a
 l'écrit de la biographie, donc je
 l'écrit de sorte avec sympathie
 la parfaite politesse et la parfaite
 bonne foi.

Monday, motions, but these were
a waste, for the same reasons
as the same day in the past.
Rising clouds

VICTOR HUGO

Date, 1860.

British Museum, Add MS 15,510





FRIEDRICH WILHELM BESSEL, WHO FIRST
MEASURED THE DISTANCE OF A STAR



Countess
John Dalton

III

A CENTURY'S PROGRESS IN CHEMISTRY

SMALL beginnings have great endings—sometimes. As a case in point, note what came of the small original effort of a self-trained back-country Quaker youth named John Dalton, who along toward the close of the eighteenth century became interested in the weather, and was led to construct and use a crude rain-gage to test the amount of the waterfall. The simple experiments thus inaugurated led to no fewer than two hundred thousand recorded observations regarding the weather, which formed the basis for some of the most epochal discoveries in meteorology.

But this was only a beginning. The simple rain-gage pointed the way to the most important generalization of our century in a field of science with which, to the casual observer, it might seem to have no alliance whatever. The wonderful theory of atoms, on which the whole gigantic structure of modern chemistry is founded, was the logical outgrowth, in the mind of John Dalton, of those early studies in meteorology.

The way it happened was this: From studying the rainfall, Dalton turned naturally to the complementary process of evaporation. He was soon led to believe that vapor exists in the atmosphere as an independent gas. But since two bodies cannot occupy the same space at the same time, this implies that the various atmospheric gases are really composed of discrete particles. These ultimate particles are so small that we cannot see them

—cannot, indeed, more than vaguely imagine them—yet each particle of vapor, for example, is just as much a portion of water as if it were a drop out of the ocean.

But again, water is a compound substance, for it may be separated, as Cavendish had shown, into the two elementary substances hydrogen and oxygen. Hence the atom of water must be composed of two lesser atoms joined together. Imagine an atom of hydrogen and one of oxygen. Unite them, and we have an atom of water; sever them, and the water no longer exists; but whether united or separate the atoms of hydrogen and of oxygen remain hydrogen and oxygen and nothing else. Differently mixed together or united, atoms produce different gross substances; but the elementary atoms never change their chemical nature—their distinct personality.

It was about the year 1803 that Dalton first gained a full grasp of the conception of the chemical atom. At once he saw that the hypothesis, if true, furnished a marvelous key to secrets of matter hitherto insoluble—questions relating to the relative proportions of the atoms themselves.

It is known, for example, that a certain bulk of hydrogen gas unites with a certain bulk of oxygen gas to form water. If it be true that this combination consists essentially of the union of atoms one with another (each single atom of hydrogen united to a single atom of oxygen), then the relative weights of the original masses of hydrogen and of oxygen must be also the relative weights of each of their respective atoms. If one pound of hydrogen unites with five and one-half pounds of oxygen (as, according to Dalton's experiments, it did), then the weight of the oxygen atom must be five and one-half times that of the hydrogen atom. Other compounds may plainly be tested in the same way.

Dalton made numerous tests before he published his theory. He found that hydrogen enters into compounds in smaller proportions than any other element known to him, and so, for convenience, determined to take the weight of the hydrogen atom as unity. The atomic weight of oxygen then becomes (as given in Dalton's first table of 1803) 5.5; that of water (hydrogen plus oxygen) being of course 6.5. The atomic weights of about a score of substances are given in Dalton's first paper, which was read before the Literary and Philosophical Society of Manchester, October 21, 1803.

I wonder if Dalton himself, great and acute intellect tho he had, suspected, when he read that paper, that he was inaugurating one of the most fertile movements ever entered on in the whole history of science?

Be that as it may, it is certain enough that Dalton's contemporaries were at first little impressed with the novel atomic theory. Just at this time, as it chanced, a dispute was waging in the field of chemistry regarding a matter of empirical fact which must necessarily be settled before such a theory as Dalton's could even hope for a hearing. This was the question whether or not chemical elements unite with one another always in definite proportions. Berthollet, the great co-worker with Lavoisier, and then the most authoritative of living chemists, contended that substances combine in almost indefinitely graded proportions between fixed extremes. He held that solution is really a form of chemical combination—a position which, if accepted, left no room for argument.

But this contention of the master was most actively disputed, in particular by Louis Joseph Proust, and all chemists of repute were obliged to take sides with one or the other. For a time the authority of Berthollet



JOSEPH LOUIS GAY-LUSSAC

held out against the facts, but at last accumulated evidence told for Proust and his followers, and toward the close of the first decade of the nineteenth century it came to be generally conceded that chemical elements combine in fixed and definite proportions.

More than that. As the analysts were led to weigh carefully the quantities of combining elements, it was observed that the proportions are not only definite, but that they bear a very curious relation to one another. If element A combines with two different proportions of element B to form two compounds, it appeared that the weight of the larger quantity of B is an exact multiple of that of the smaller quantity. This curious relation was noticed by Dr. Wollaston, one of the most accurate of observers, and a little later it was confirmed by Johan Jakob Berzelius, the great Swedish chemist, who was to be a dominating influence in the chemical world for a generation to come.

But this combination of elements in numerical proportions was exactly what Dalton had noticed as early as 1802, and what had led him directly to the atomic weights. So the confirmation of this essential point by chemists of such authority gave the strongest confirmation to the atomic theory.

During these same years the rising authority of the French chemical world, Joseph Louis Gay-Lussac, was conducting experiments with gases, which he had undertaken at first in conjunction with Humboldt, but which later on were conducted independently. In 1809, the next year after the publication of the first volume of Dalton's *New System of Chemical Philosophy*, Gay-Lussac published the results of his observations, and among other things brought out the remarkable fact that gases, under the same conditions as to temperature

and pressure, combine always in definite numerical proportions as to volume. Exactly two volumes of hydrogen, for example, combine with one volume of oxygen to form water. Moreover, the resulting compound gas always bears a simple relation to the combining volumes. In the case just cited the union of two volumes of hydrogen and one of oxygen results in precisely two volumes of water vapor.

Naturally enough the champions of the atomic theory seized upon these observations of Gay-Lussac as lending strong support to their hypothesis—all of them, that is, but the curiously self-reliant and self-sufficient author of the atomic theory himself, who declined to accept the observations of the French chemist as valid. Yet the observations of Gay-Lussac were correct, as countless chemists since then have demonstrated anew, and his theory of combination by volumes became one of the foundation-stones of the atomic theory, despite the opposition of the author of that theory.

The true explanation of Gay-Lussac's law of combination by volumes was thought out almost immediately by an Italian savant, Amadeo Avogadro, and expressed in terms of the atomic theory. The fact must be, said Avogadro, that under similar physical conditions every form of gas contains exactly the same number of ultimate particles in a given volume. Each of these ultimate physical particles may be composed of two or more atoms (as in the case of water vapor), but such a compound atom conducts itself as if it were a simple and indivisible atom, as regards the amount of space that separates it from its fellows under given conditions of pressure and temperature.

The compound atom, composed of two or more elementary atoms, Avogadro proposed to distinguish, for

purposes of convenience, by the name molecule. It is to the molecule, considered as the unit of physical structure, that Avogadro's law applies.

This vastly important distinction between atoms and molecules, implied in the law just expressed, was published in 1811. Four years later, the famous French physicist Ampère outlined a similar theory, and utilized the law in his mathematical calculations. And with that the law of Avogadro dropped out of sight for a full generation. Little suspecting that it was the very key to the inner mysteries of the atoms for which they were seeking, the chemists of the time cast it aside, and let it fade from the memory of their science.

This, however, was not strange, for of course the law of Avogadro is based on the atomic theory, and in 1811 the atomic theory was itself still being weighed in the balance. The law of multiple proportions found general acceptance as an empirical fact; but many of the leading lights of chemistry still looked askance at Dalton's explanation of this law. Thus Wollaston, tho from the first he inclined to acceptance of the Daltonian view, cautiously suggested that it would be well to use the non-committal word "equivalent" instead of "atom"; and Davy, for a similar reason, in his book of 1812, speaks only of "proportions," binding himself to no theory as to the nature of these proportions.

At least two great chemists of the time, however, adopted the atomic view with less reservation. One of these was Thomas Thomson, professor at Edinburgh, who in 1807 had given an outline of Dalton's theory in a widely circulated book, which first brought the theory to the general attention of the chemical world. The other, and even more noted advocate of the atomic theory was Johan Jakob Berzelius. This great Swedish

chemist at once set to work to put the atomic theory to such tests as might be applied in the laboratory. He was an analyst of the utmost skill, and for years he devoted himself to the determination of the combining weights, "equivalents," or "proportions" of the different elements. These determinations, in so far as they were accurately made, were simple expressions of empirical facts, independent of any theory; but gradually it became more and more plain that these facts all harmonized with the atomic theory of Dalton.

So by common consent the proportionate combining weights of the elements came to be known as atomic weights—the name Dalton had given them from the first—and the tangible conception of the chemical atom as a body of definite constitution and weight gained steadily in favor.

From the outset the idea had had the utmost tangibility in the mind of Dalton. He had all along represented the different atoms by geometrical symbols—as a circle for oxygen, a circle enclosing a dot for hydrogen, and the like—and had represented compounds by placing these symbols of the elements in juxtaposition.

Berzelius proposed to improve upon this method by substituting for the geometrical symbol the initial of the Latin name of the element represented—O for oxygen, H for hydrogen, and so on—a numerical coefficient to follow the letter as an indication of the number of atoms present in any given compound. This simple system soon gained general acceptance, and every schoolboy now is aware that H_2O is the chemical way of expressing the union of two atoms of hydrogen with one of oxygen to form a molecule of water. But such a formula would have had no meaning for the wisest chemist before the day of Berzelius.



JACOB BERZELIUS

The universal fame of the great Swedish authority served to give general currency to his symbols and atomic weights, and the new point of view thus developed led presently to two important discoveries which removed the last lingering doubts as to the validity of the atomic theory. In 1819 two French physicists, Dulong and Petit, while experimenting with heat, discovered that the specific heats of solids (that is to say, the amount of heat required to raise the temperature of a given mass to a given degree) vary inversely as their atomic weights. In the same year Eilhard Mitscherlich, a German investigator, observed that compounds having the same number of atoms to the molecule are disposed to form the same angles of crystallization—a property which he called isomorphism.

Here, then, were two utterly novel and independent sets of empirical facts which harmonize strangely with the supposition that substances are composed of chemical atoms of a determinate weight. This surely could not be coincidence—it tells of law. And so as soon as the claims of Dulong and Petit and of Mitscherlich had been substantiated by other observers, the laws of the specific heat of atoms, and of isomorphism, took their place as new levers of chemical science.

With the aid of these new tools an impregnable breastwork of facts was soon piled about the atomic theory. And John Dalton, the author of that theory, plain, provincial Quaker, working on to the end in semi-retirement, became known to all the world and for all time as a master of masters.

During those early years of the century, when Dalton was grinding away at chemical fact and theory in his obscure Manchester laboratory, another Englishman held the attention of the chemical world with a series

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of the most brilliant and widely heralded researches. Humphry Davy had come to London in 1801, at the instance of Count Rumford, to assume the chair of chemical philosophy in the Royal Institution, which the famous American had just founded.

Here, under Davy's direction, the largest voltaic battery yet constructed had been put in operation, and with its aid the brilliant young experimenter was expected almost to perform miracles. And indeed he scarcely disappointed the expectation, for with the aid of his battery he transformed so familiar a substance as common potash into a metal which was not only so light that it floated on water, but possessed the seemingly miraculous property of bursting into flames as soon as it came in contact with that fire-quenching liquid. If this were not a miracle, it had for the popular eye all the appearance of the miraculous.

What Davy really had done was to decompose the potash, which hitherto had been supposed to be elementary, liberating its oxygen, and thus isolating its metallic base, which he named potassium. The same thing was done with soda, and the closely similar metal sodium was discovered—metals of a unique type, possessed of a strange avidity for oxygen, and capable of seizing on it even when it is bound up in the molecules of water.

Considered as mere curiosities, these discoveries were interesting, but aside from that they were of great theoretical importance, because they showed the compound nature of some familiar chemicals that had been regarded as elements. Several other elementary earths met the same fate when subjected to the electrical influence, the metals barium, calcium, and strontium being thus discovered. Thereafter Davy always referred to the supposed elementary substances (including oxygen,

hydrogen, and the rest) as "undecompounded" bodies, because they resisted all efforts to decompose them.

Another and even more important theoretical result that flowed from Davy's experiments during this first decade of the century was the proof that no elementary substances other than hydrogen and oxygen are produced when pure water is decomposed by the electric current. It was early noticed by Davy and others that when a strong current is passed through water, alkalies appear at one pole of the battery and acids at the other, and this tho the water used were absolutely pure. This seemingly told of the creation of elements—a transmutation but one step removed from the creation of matter itself—under the influence of the new "force."

It was one of Davy's greatest triumphs to prove, in the series of experiments recorded in his famous Bakerian lecture of 1806, that the alleged creation of elements did not take place, the substances found at the poles of the battery having been dissolved from the walls of the vessels in which the water experimented upon had been placed. Thus the same implement which had served to give a certain philosophical warrant to the fading dreams of alchemy banished those dreams peremptorily from the domain of science.

Tho the presence of the alkalies and acids in the water was explained, however, their respective migrations to the negative and positive poles of the battery remained to be accounted for. Davy's classical explanation assumed that different elements differ among themselves as to their electrical properties, some being positively, others negatively, electrified. Electricity and "chemical affinity," he said, apparently are manifestations of the same force, acting in the one case on

masses, in the other on particles. Electro-positive particles unite with electro-negative particles to form chemical compounds, in virtue of the familiar principle that opposite electricities attract one another. When compounds are decomposed by the battery, this mutual attraction is overcome by the stronger attraction of the poles of the battery itself.

This theory of binary composition of all chemical compounds, through the union of electro-positive and electro-negative atoms or molecules, was extended by Berzelius, and made the basis of his famous system of theoretical chemistry. This theory held that all inorganic compounds, however complex their composition, are essentially composed of such binary combinations. For many years this view enjoyed almost undisputed sway. It received what seemed strong confirmation when Faraday showed the definite connection between the amount of electricity employed and the amount of decomposition produced in the so-called electrolyte. But its claims were really much too comprehensive, as subsequent discoveries proved.

When Berzelius first promulgated his binary theory he was careful to restrict its unmodified application to the compounds of the inorganic world. At that time, and for a long time thereafter, it was supposed that substances of organic nature had some properties that kept them aloof from the domain of inorganic chemistry. It was little doubted that a so-called "vital force" operated here, replacing or modifying the action of ordinary "chemical affinity." It was, indeed, admitted that organic compounds are composed of familiar elements—chiefly carbon, oxygen, hydrogen, and nitrogen—but these were supposed to be united in ways that could not be imitated in the domain of the non-living.

It was regarded almost as an axiom of chemistry that no organic compound whatever could be put together from its elements—synthesized—in the laboratory. To effect the synthesis of even the simplest organic compound it was thought that the “vital force” must be in operation.

Therefore a veritable sensation was created in the chemical world when, in the year 1828, it was announced that the young German chemist Friedrich Wöhler, former pupil of Berzelius, and already known as a coming master, had actually synthesized the well-known organic product urea in his laboratory at Sacrow. The “exception which proves the rule” is something never heard of in the domain of logical science. Natural law knows no exceptions. So the synthesis of a single organic compound sufficed at a blow to break down the chemical barrier which the imagination of the fathers of the science had erected between animate and inanimate nature.

Thenceforth the philosophical chemist would regard the plant and animal organisms as chemical laboratories in which conditions are peculiarly favorable for building up complex compounds of a few familiar elements, under the operation of universal chemical laws. The chimera “vital force” could no longer gain recognition in the domain of chemistry.

Now a wave of interest in organic chemistry swept over the chemical world, and soon the study of carbon compounds became as much the fashion as electrochemistry had been in the preceding generation.

Foremost among the workers who rendered this epoch of organic chemistry memorable were Justus Liebig in Germany and Jean Baptiste André Dumas in France, and their respective pupils, Charles Frédéric Gerhardt

and Augustus Laurent. Wöhler, too, must be named in the same breath, as also must Louis Pasteur, who, tho somewhat younger than the others, came upon the scene in time to take chief part in the most important of the controversies that grew out of their labors.

Several years earlier than this the way had been paved for the study of organic substances by Gay-Lussac's discovery, made in 1815, that a certain compound of carbon and nitrogen, which he named cyanogen, has a peculiar degree of stability which enables it to retain its identity and enter into chemical relations after the manner of a simple body. A year later Ampère discovered that nitrogen and hydrogen, when combined in certain proportions to form what he called ammonium, have the same property. Berzelius had seized upon this discovery of the compound radical, as it was called, because it seemed to lend aid to his dualistic theory. He conceived the idea that all organic compounds are binary unions of various compound radicals with an atom of oxygen, announcing this theory in 1818.

Ten years later, Liebig and Wöhler undertook a joint investigation which resulted in proving that compound radicals are indeed very abundant among organic substances. Thus the theory of Berzelius seemed to be substantiated, and organic chemistry came to be defined as the chemistry of compound radicals.

But even in the day of its seeming triumph the dualistic theory was destined to receive a rude shock. This came about through the investigations of Dumas, who proved that in a certain organic substance an atom of hydrogen may be removed and an atom of chlorine substituted in its place, without destroying the integrity of the original compound—much as a child might sub-

stitute one block for another in its play-house. Such a substitution would be quite consistent with the dualistic theory, were it not for the very essential fact that hydrogen is a powerfully electro-positive element, while chlorine is as strongly electro-negative. Hence the compound radical which united successively with these two elements must itself be at one time electro-positive, at another electro-negative — a seeming inconsistency which threw the entire Berzelian theory into disfavor.

In its place there was elaborated, chiefly through the efforts of Laurent and Gerhardt, a conception of the molecule as a unitary structure, built up through the aggregation of various atoms, in accordance with "elective affinities" whose nature was not yet understood. A doctrine of "nuclei" and a doctrine of "types" of molecular structure were much exploited, and, like the doctrine of compound radicals, became useful as aids to memory and guides for the analyst, tho by no means penetrating the mysteries of chemical affinity. They are classifications rather than explanations of chemical unions. But at least they served an important purpose in giving definiteness to the idea of a molecular structure built of atoms as the basis of all substances.

Now at last the word molecule came to have a distinct meaning, as distinct from "atom," in the minds of the generality of chemists, as it had had for Avogadro a third of a century before. Avogadro's hypothesis that there are equal numbers of these molecules in equal volumes of gases, under fixed conditions, was revived by Gerhardt, and a little later, under the championship of Cannizzaro, was exalted to the plane of a fixed law. Thenceforth the conception of the molecule was to be as dominant a thought in chemistry as the idea of the atom had become in a previous epoch.

In many cases the chemists had supposed themselves dealing with atoms as units where the true unit was the molecule. In the case of elementary gases, such as hydrogen and oxygen, for example, the law of equal numbers of molecules in equal spaces made it clear that the atoms do not exist isolated, as had been supposed. Since two volumes of hydrogen unite with one volume of oxygen to form two volumes of water vapor, the simplest mathematics shows, in the light of Avogadro's law, not only that each molecule of water must contain two hydrogen atoms (a point previously in dispute), but that the original molecules of hydrogen and oxygen must have been composed in each case of two atoms—else how could one volume of oxygen supply an atom for every molecule of two volumes of water?

What, then, does this imply? Why, that the elementary atom has an avidity for other atoms, a longing for companionship, an "affinity"—call it what you will—which is bound to be satisfied if other atoms are in the neighborhood. Placed solely among atoms of its own kind, the oxygen atom seizes on a fellow oxygen atom, and in all their mad dancings these two mates cling together—possibly revolving about one another in miniature planetary orbits. Precisely the same thing occurs among the hydrogen atoms

But now suppose the various pairs of oxygen atoms come near other pairs of hydrogen atoms (under proper conditions which need not detain us here), then each oxygen atom loses its attachment for its fellow, and flings itself madly into the circuit of one of the hydrogen couplets, and—presto!—there are only two molecules for every three there were before, and free oxygen and hydrogen have become water.

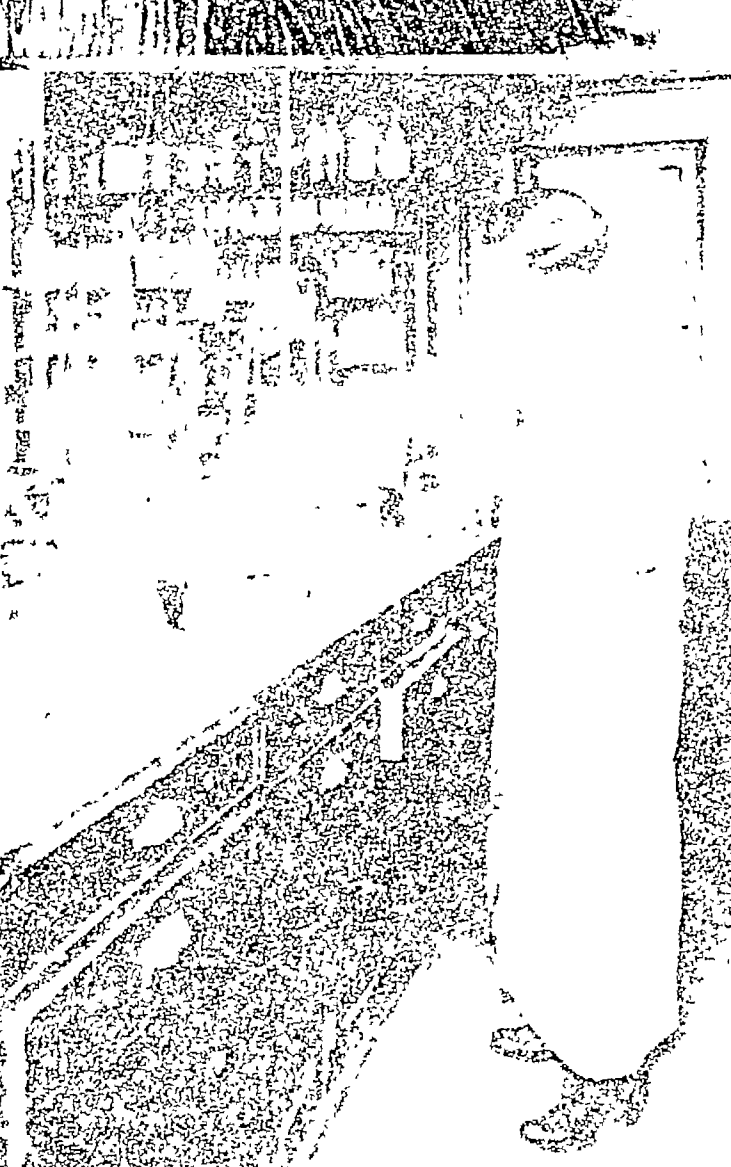
The whole process, stated in chemical phraseology,

is summed up in the statement that under the given conditions the oxygen atoms had a greater affinity for the hydrogen atoms than for one another.

As chemists studied the actions of various kinds of atoms, in regard to their unions with one another to form molecules, it gradually dawned upon them that not all elements are satisfied with the same number of companions. Some elements ask only one, and refuse to take more; while others link themselves, when occasion offers, with two, three, four, or more. Thus we saw that oxygen forsook a single atom of its own kind and linked itself with two atoms of hydrogen. Clearly, then, the oxygen atom, like a creature with two hands, is able to clutch two other atoms. But we have no proof that under any circumstances it could hold more than two. Its affinities seem satisfied when it has two bonds. But, on the other hand, the atom of nitrogen is able to hold three atoms of hydrogen, and does so in the molecule of ammonium (NH_3); while the carbon atom can hold four atoms of hydrogen or two atoms of oxygen.

Evidently, then, one atom is not always equivalent to another atom of a different kind in combining powers. A recognition of this fact by Frankland about 1852, and its further investigation by others (notably A. Kekulé and A. S. Couper), led to the introduction of the word equivalent into chemical terminology in a new sense, and in particular to an understanding of the affinities or "valency" of different elements, which proved of the most fundamental importance.

Thus it was shown that, of the four elements that enter most prominently into organic compounds, hydrogen can link itself with only a single bond to any other element—it has, so to speak, but a single hand



with which to grasp—while oxygen has capacity for two bonds, nitrogen for three (possibly for five), and carbon for four. The words monovalent, divalent, trivalent, tetravalent, etc., were coined to express this most important fact, and the various elements came to be known as monads, diads, triads, etc. Just why different elements should differ thus in valency no one as yet knows, it is an empirical fact that they do. And once the nature of any element has been determined as regards its valency, a most important insight into the possible behavior of that element has been secured.

Thus a consideration of the fact that hydrogen is monovalent, while oxygen is divalent, makes it plain that we must expect to find no more than three compounds of these two elements, namely, $\text{H}-\text{O}-$ (written HO by the chemist, and called hydroxyl); $\text{H}-\text{O}-\text{H}$ (H_2O , or water), and $\text{H}-\text{O}-\text{O}-\text{H}$ (H_2O_2 , or hydrogen peroxid). It will be observed that in the first of these compounds the atom of oxygen stands, so to speak, with one of its hands free, eagerly reaching out, therefore, for another companion, and hence, in the language of chemistry, forming an unstable compound. Again, in the third compound, tho all hands are clasped, yet one pair links oxygen with oxygen; and this also must be an unstable union, since the avidity of an atom for its own kind is relatively weak. Thus the well-known properties of hydrogen peroxid are explained, its easy decomposition, and the eagerness with which it seizes upon the elements of other compounds.

But the molecule of water, on the other hand, has its atoms arranged in a state of stable equilibrium, all their affinities being satisfied. Each hydrogen atom has satisfied its own affinity by clutching the oxygen atom; and the oxygen atom has both its bonds satisfied by clutch

ing back at the two hydrogen atoms. Therefore the trio, linked in this close bond, have no tendency to reach out for any other companion, nor, indeed, any power to hold another should it thrust itself upon them. They form a "stable" compound, which under all ordinary circumstances will retain its identity as a molecule of water, even tho the physical mass of which it is a part changes its condition from a solid to a gas—from ice to vapor.

But a consideration of this condition of stable equilibrium in the molecule at once suggests a new question: How can an aggregation of atoms, having all their affinities satisfied, take any further part in chemical reactions? Seemingly such a molecule, whatever its physical properties, must be chemically inert, incapable of any atomic readjustments. And so in point of fact it is, so long as its component atoms cling to one another unremittingly. But this, it appears, is precisely what the atoms are little prone to do. It seems that they are fickle to the last degree in their individual attachments, and are as prone to break away from bondage as they are to enter into it. Thus the oxygen atom which has just flung itself into the circuit of two hydrogen atoms, the next moment flings itself free again and seeks new companions. It is for all the world like the incessant change of partners in a rollicking dance.

This incessant dissolution and reformation of molecules in a substance which as a whole remains apparently unchanged was first fully appreciated by Ste.-Claire Deville, and by him named dissociation. It is a process which goes on much more actively in some compounds than in others, and very much more actively under some physical conditions (such as increase of temperature) than under others. But apparently no

substances at ordinary temperatures, and no temperature above the absolute zero, are absolutely free from its disturbing influence. Hence it is that molecules having all the valency of their atoms fully satisfied do not lose their chemical activity—since each atom is momentarily free in the exchange of partners, and may seize upon different atoms from its former partners, if those it prefers are at hand.

While, however, an appreciation of this ceaseless activity of the atom is essential to a proper understanding of its chemical efficiency, yet from another point of view the "saturated" molecule—that is, the molecule whose atoms have their valency all satisfied—may be thought of as a relatively fixed or stable organism. Even tho it may presently be torn down, it is for the time being a completed structure; and a consideration of the valency of its atoms gives the best clue that has hitherto been obtainable as to the character of its architecture

How important this matter of architecture of the molecule—of space relations of the atoms—may be was demonstrated as long ago as 1823, when Liebig and Wöhler proved, to the utter bewilderment of the chemical world, that two substances may have precisely the same chemical constitution—the same number and kind of atoms—and yet differ utterly in physical properties. The word isomerism was coined by Berzelius to express this anomalous condition of things, which seemed to negative the most fundamental truths of chemistry. Naming the condition by no means explained it, but the fact was made clear that something besides the mere number and kind of atoms is important in the architecture of a molecule.

It became certain that atoms are not thrown together



haphazard to build a molecule, any more than bricks are thrown together at random to form a house.

How delicate may be the gradations of architectural design in building a molecule was well illustrated about 1850, when Pasteur discovered that some carbon compounds—as certain sugars—can only be distinguished from one another, when in solution, by the fact of their twisting or polarizing a ray of light to the left or to the right, respectively. But no inkling of an explanation of these strange variations of molecular structure came until the discovery of the law of valency. Then much of the mystery was cleared away; for it was plain that since each atom in a molecule can hold to itself only a fixed number of other atoms, complex molecules must have their atoms linked in definite chains or groups. And it is equally plain that where the atoms are numerous, the exact plan of grouping may sometimes be susceptible of change without doing violence to the law of valency. It is in such cases that isomerism is observed to occur.

By paying constant heed to this matter of the affinities, chemists are able to make diagrammatic pictures of the plan of architecture of any molecule whose composition is known. In the simple molecule of water (H_2O), for example, the two hydrogen atoms must have released one another before they could join the oxygen, and the manner of linking must apparently be that represented in the graphic formula $\text{H}-\text{O}-\text{H}$. With molecules composed of a large number of atoms, such graphic representation of the scheme of linking is of course increasingly difficult, yet, with the affinities for a guide, it is always possible.

Of course no one supposes that such a formula, written in a single plane, can possibly represent the true

architecture of the molecule: it is at best suggestive or diagrammatic rather than pictorial. Nevertheless, it affords hints as to the structure of the molecule such as the fathers of chemistry would not have thought it possible ever to attain.

These utterly novel studies of molecular architecture may seem at first sight to take from the atom much of its former prestige as the all-important personage of the chemical world. Since so much depends upon the mere position of the atoms, it may appear that comparatively little depends upon the nature of the atoms themselves. But such a view is incorrect, for on closer consideration it will appear that at no time has the atom been seen to renounce its peculiar personality. Within certain limits the character of a molecule may be altered by changing the positions of its atoms (just as different buildings may be constructed of the same bricks), but these limits are sharply defined, and it would be as impossible to exceed them as it would be to build a stone building with bricks. From first to last the brick remains a brick, whatever the style of architecture it helps to construct; it never becomes a stone. And just as closely does each atom retain its own peculiar properties, regardless of its surroundings.

Thus, for example, the carbon atom may take part in the formation at one time of a diamond, again of a piece of coal, and yet again of a particle of sugar, of wood fiber, of animal tissue, or of a gas in the atmosphere; but from first to last—from glass-cutting gem to intangible gas—there is no demonstrable change whatever in any single property of the atom itself. So far as we know, its size, its weight, its capacity for vibration or rotation, and its inherent affinities, remain absolutely unchanged throughout all these varying fortunes of po-



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sition and association. And the same thing is true of every atom of all of the eighty odd elementary substances with which the modern chemist is acquainted. Every one appears always to maintain its unique integrity, gaining nothing and losing nothing.

All this being true, it seemed as if the position of the Daltonian atom as a primordial bit of matter, indestructible and non-transmutable, had been put to the test by chemistry and not found wanting. Since those early days of the century when the electric battery performed its miracles and seemingly reached its limitations in the hands of Davy, many new elementary substances had been discovered, but no single element had been displaced from its position as an undecomposable body. Rather had the analyses of the chemist seemed to make it more and more certain that all elementary atoms were in truth what John Herschel called them, "manufactured articles" — primordial, changeless, indestructible. And so it was to continue to seem until well on into the twentieth century.

Yet, oddly enough, it chanced that hand in hand with the experiments leading to such a goal have gone other experiments and speculations of exactly the opposite tenor. In each generation there were chemists among the leaders of their science who refused to admit that the so-called elements were really elements at all in any final sense, and who sought eagerly for proof which might warrant their skepticism. The first bit of evidence tending to support this view was furnished by an English physician, Dr. William Prout, who in 1815 called attention to a curious relation to be observed between the atomic weights of the various elements.

Accepting the figures given by the authorities of the time (notably Thomson and Berzelius), it appeared that

a strikingly large proportion of the atomic weights were exact multiples of the weight of hydrogen, and that others differed so slightly that errors of observation might explain the discrepancy. Prout felt that this could not be accidental, and he could think of no tenable explanation, unless it be that the atoms of the various alleged elements are made up of different fixed numbers of hydrogen atoms. Could it be that the one true element—the one primal matter—is hydrogen, and that all other forms of matter are but compounds of this original substance?

Prout advanced this startling idea at first tentatively, in an anonymous publication; but afterwards he espoused it openly and urged its tenability. Coming just after Davy's dissociation of some supposed elements, the idea proved alluring, and for a time gained such popularity that chemists were disposed to round out the observed atomic weights of all elements into whole numbers.

But presently renewed determinations of the atomic weights seemed to discountenance this practise, and Prout's alleged law fell into disrepute. It was revived, however, about 1840, by Dumas, whose great authority secured it a respectful hearing, and whose careful re-determination of the weight of carbon, making it exactly twelve times that of hydrogen, aided the cause.

Subsequently Stas, a pupil of Dumas, undertook a long series of determinations of atomic weights, with the expectation of confirming the Proutian hypothesis. But his results seemed to disprove the hypothesis, for the atomic weights of many elements differed from whole numbers by more, it was thought, than the limits of error of the experiments. It is noteworthy, however, that the confidence of Dumas was not shaken, tho he was led to modify the hypothesis, and, in accordance

with previous suggestions of Clark and of Marignac, to recognize as the primordial element, not hydrogen itself, but an atom half the weight, or even one-fourth the weight, of that of hydrogen, of which primordial atom the hydrogen atom itself is compounded. But even in this modified form the hypothesis found great opposition from experimental observers.

In 1864, however, a novel relation between the weights of the elements and their other characteristics was called to the attention of chemists by Professor John A. R. Newlands, of London, who had noticed that if the elements are arranged serially in the numerical order of their atomic weights, there is a curious recurrence of similar properties at intervals of eight elements. This so-called "law of octaves" attracted little immediate attention, but the facts it connotes soon came under the observation of other chemists, notably of Professors Gustav Hinrichs in America, Dmitri Mendeléeff in Russia, and Lothar Meyer in Germany. Mendeléeff gave the discovery fullest expression, expositing it in 1869 under the title of "periodic law."

Tho this early exposition of what has since been admitted to be a most important discovery was very fully outlined, the generality of chemists gave it little heed till a decade or so later, when three new elements, gallium, scandium, and germanium, were discovered, which, on being analyzed, were quite unexpectedly found to fit into three gaps which Mendeléeff had left in his periodic scale. In effect, the periodic law had enabled Mendeléeff to predicate the existence of the new elements years before they were discovered. Surely a system that leads to such results is no mere vagary. So very soon the periodic law took its place as one of the most important generalizations of chemical science.



This law of periodicity was put forward as an expression of observed relations independent of hypothesis; but of course the theoretical bearings of these facts could not be overlooked. As Professor J. H. Gladstone has said, it forces upon us "the conviction that the elements are not separate bodies created without reference to one another, but that they have been originally fashioned, or have been built up, from one another, according to some general plan." It is but a short step from that proposition to the Proutian hypothesis.

But the atomic weights are not alone in suggesting the compound nature of the alleged elements. Evidence of a totally different kind has contributed to the same end, from a source that could hardly have been imagined when the Proutian hypothesis was formulated, through the addition of a novel weapon to the armamentarium of the chemist—the spectroscope. The perfection of this instrument, in the hands of two German scientists, Gustav Robert Kirchhoff and Robert Wilhelm Bunsen, came about through the investigation, toward the middle of the century, of the meaning of the dark lines which had been observed in the solar spectrum by Fraunhofer as early as 1815, and by Wollaston a decade earlier.

It was suspected by Stokes and by Fox Talbot in England, but first brought to demonstration by Kirchhoff and Bunsen, that these lines, which were known to occupy definite positions in the spectrum, are really indicative of particular elementary substances. By means of the spectroscope, which is essentially a magnifying lens attached to a prism of glass, it is possible to locate the lines with great accuracy, and it was soon shown that here was a new means of chemical analysis of the most exquisite delicacy.

As to Brougham's feelings towards myself, I know and have known for a long time that he hates me. If during the last ten years I have gained any reputation either in politics or in letters, if I have had any success in life, it has been without his help or countenance, and often in spite of his utmost exertions to keep me down. It is strange that he should be surprised at my not calling on him since my return. I did not call on him when I was away. When he was Chancellor and I was in office I never once attended his house. It would be strange indeed if now, when he is squandering the remains of his public character in an attempt to ruin the party of which he was a member then and of which I am a member still, I should begin to pay court to him. For the sake of the long intimacy which subsisted between him and my father, and of the mutual good offices which passed between

THOMAS BABINGTON MACAULAY.

Died, 1858.

British Museum Add. MS. 34619.



It was found, for example, that the spectroscope could detect the presence of a quantity of sodium so infinitesimal as the one two-hundred-thousandth of a grain. But what was even more important, the spectroscope put no limit upon the distance of location of the substance it tested, provided only that sufficient light came from it. The experiments it recorded might be performed in the sun, or in the most distant stars or nebulae; indeed, one of the earliest feats of the instrument was to wrench from the sun the secret of his chemical constitution.

To render the utility of the spectroscope complete, however, it was necessary to link with it another new chemical agency, namely, photography. This now familiar process is based on the property of light to decompose certain unstable compounds of silver, and thus alter their chemical composition. We have seen that Davy and Wedgwood barely escaped the discovery of the value of the photographic method. Their successors quite overlooked it until about 1826, when Louis J. M. Daguerre, the French chemist, took the matter in hand, and after many years of experimentation brought it to relative perfection in 1839, in which year the famous daguerreotype first brought the matter to popular attention. In the same year Mr. Fox Talbot read a paper on the subject before the Royal Society, and soon afterwards the efforts of Herschel and numerous other natural philosophers contributed to the advancement of the new method.

In 1843 Dr. John W. Draper, the famous English-American chemist and physiologist, showed that by photography the Fraunhofer lines in the solar spectrum might be mapped with absolute accuracy; also proving that the silvered film revealed many lines invisible to

the unaided eye. The value of this method of observation was recognized at once, and, as soon as the spectroscope was perfected, the photographic method, in conjunction with its use, became invaluable to the chemist. By this means comparisons of spectra may be made with a degree of accuracy not otherwise obtainable; and in case of the stars, whole clusters of spectra may be placed on record at a single observation.



LABORATORY WORKERS

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Chapter

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[illegible]

SIR WALTER SCOTT

Date: 1/25/21

British Museum, London MS 1461





GREGOR MENDEL

IV

A CENTURY'S PROGRESS IN BIOLOGY

WHEN Coleridge said of Humphry Davy that he might have been the greatest poet of his time had he not chosen rather to be the greatest chemist, it is possible that the enthusiasm of the friend outweighed the caution of the critic. But however that may be, it is beyond dispute that the man who actually was the greatest poet of that time might easily have taken the very highest rank as a scientist had not the Muse distracted his attention. Indeed, despite these distractions, Johann Wolfgang von Goethe achieved successes in the field of pure science that would insure permanent recognition for his name had he never written a stanza of poetry. Such is the versatility that marks the highest genius.

It was in 1790 that Goethe published the work that laid the foundations of his scientific reputation — the work on the *Metamorphoses of Plants*, in which he advanced the novel doctrine that all parts of the flower are modified or metamorphosed leaves. This was followed presently by an extension of the doctrine of metamorphosis to the animal kingdom, in the doctrine which Goethe and Oken advanced independently, that the vertebrate skull is essentially a modified and developed vertebra. These were conceptions worthy of a poet; impossible, indeed, for any mind that had not the poetic faculty of correlation. But in this case the poet's vision was prophetic of a future view of the most prosaic science. The doctrine of metamorphosis of parts soon came

to be regarded as a fundamental feature in the science of living things.

But the doctrine had implications that few of its early advocates realized. If all the parts of a flower—sepal, petal, stamen, pistil, with their countless deviations of contour and color—are but modifications of the leaf, such modification implies a marvelous differentiation and development. To assert that a stamen is a metamorphosed leaf means, if it means anything, that in the long sweep of time the leaf has by slow or sudden gradations changed its character through successive generations, until the offspring, so to speak, of a true leaf has become a stamen. But if such a metamorphosis as this is possible—if the seemingly wide gap between leaf and stamen may be spanned by the modification of a line of organisms—where does the possibility of modification of organic type find its bounds?

Why may not the modification of parts go on along devious lines until the remote descendants of an organism are utterly unlike that organism? Why may we not thus account for the development of various species of beings all sprung from one parent stock? That too is a poet's dream; but is it only a dream? Goethe thought not.

Out of his studies of metamorphosis of parts there grew in his mind the belief that the multitudinous species of plants and animals about us have been evolved from fewer and fewer earlier parent types, like twigs of a giant tree drawing their nurture from the same primal root. It was a bold and revolutionary thought; and the world regarded it as but the vagary of a poet.

Just at the time when this thought was taking form in Goethe's brain, the same idea was germinating in the mind of another philosopher, an Englishman of interna-

tional fame, Dr. Erasmus Darwin, who, while he lived, enjoyed the widest popularity as a poet, the rimed couplets of his *Botanic Garden* being quoted everywhere with admiration. And posterity, repudiating the verse which makes the body of the book, yet grants permanent value to the book itself, because, forsooth, its copious explanatory footnotes furnish an outline of the status of almost every department of science of the time.

But even tho he lacked the highest art of the versifier, Darwin had, beyond peradventure, the imagination of a poet coupled with profound scientific knowledge; and it was his poetic insight, correlating organisms seemingly diverse in structure, and imbuing the lowliest flower with a vital personality, which led him to suspect that there are no lines of demarcation in nature. "Can it be," he queries, "that one form of organism has developed from another; that different species are really but modified descendants of one parent stock?" The alluring thought nestled in his mind and was nurtured there, and grew into a fixed belief, which was given fuller expression in his *Zoönomia*, and in the posthumous *Temple of Nature*. But there was little proof of its validity forthcoming that could satisfy any one but a poet, and when Erasmus Darwin died, in 1802, the idea of transmutation of species was still but an unsubstantiated dream.

It was a dream, however, which was not confined to Goethe and Darwin. Even earlier the idea had come more or less vaguely to another great dreamer—and worker—of Germany, Immanuel Kant, and to several great Frenchmen, including De Maillet, Maupertuis, Robinet, and the famous naturalist Buffon—a man who had the imagination of a poet, tho his message was

couched in most artistic prose. Not long after the middle of the eighteenth century Buffon had put forward the idea of transmutation of species, and he reiterated it from time to time from then on till his death in 1788. But the time was not yet ripe for the idea of transmutation of species to burst its bonds.

And yet this idea, in a modified or undeveloped form, had taken strange hold upon the generation that was upon the scene at the close of the eighteenth century. Vast numbers of hitherto unknown species of animals had been recently discovered in previously unexplored regions of the globe, and the wise men were sorely puzzled to account for the disposal of all of these at the time of the Deluge. It simplified matters greatly to suppose that many existing species had been developed since the episode of the Ark by modification of the original pairs. The remoter bearings of such a theory were overlooked for the time, and the idea that American animals and birds, for example, were modified descendants of Old World forms—the jaguar of the leopard, the puma of the lion, and so on—became a current belief with that class of humanity who accept almost any statement as true that harmonizes with their prejudices, without realizing its implications.

Thus it is recorded with *éclat* that the discovery of the close proximity of America at the northwest with Asia removes all difficulties as to the origin of the Occidental faunas and floras, since Oriental species might easily have found their way to America on the ice, and have been modified as we find them by "the well-known influence of climate." And the persons who gave expression to this idea never dreamed of its real significance.

In truth, here was the doctrine of evolution in a nut-

shell, and, because its ultimate bearings were not clear, it seemed the most natural of doctrines. But most of the persons who advanced it would have turned from it aghast could they have realized its import. As it was, however, only here and there a man like Buffon reasoned far enough to inquire what might be the limits of such assumed transmutation; and only here and there a Darwin or a Goethe reached the conviction that there are no limits.

And even Goethe and Darwin had scarcely passed beyond that tentative stage of conviction in which they held the thought of transmutation of species as an ancillary belief, not yet ready for full exposition. There was one of their contemporaries, however, who, holding the same conception, was moved to give it full explanation. This was the friend and disciple of Buffon, Jean Baptiste de Lamarck. Possessed of the spirit of a poet and philosopher, this great Frenchman had also the widest range of technical knowledge, covering the entire field of animate nature.

The first half of Lamarck's long life was devoted chiefly to botany, in which he attained high distinction. Then, just at the beginning of the new century, he turned to zoology, in particular to the lower forms of animal life. Studying these lowly organisms, existing and fossil, he was more and more impressed with the gradations of form everywhere to be seen; the linking of diverse families through intermediate ones; and in particular with the predominance of low types of life in the earlier geological strata.

Called upon constantly to classify the various forms of life in the course of his systematic writings, he found it more and more difficult to draw sharp lines of demarcation, and at last the suspicion long harbored grew into

a settled conviction that there is really no such thing as a species of organism in nature; that "species" is a figment of the human imagination, whereas in nature there are only individuals.

That certain sets of individuals are more like one another than like other sets is of course patent, but this only means, said Lamarck, that these similar groups have had comparatively recent common ancestors, while dissimilar sets of beings are more remotely related in consanguinity. But trace back the lines of descent far enough, and all will culminate in one original stock. All forms of life whatsoever are modified descendants of an original organism. From lowest to highest, then, there is but one race, one species, just as all the multitudinous branches and twigs from one root are but one tree. For purposes of convenience of description, we may divide organisms into orders, families, genera, species, just as we divide a tree into root, trunk, branches, twigs, leaves; but in the one case, as in the other, the division is arbitrary and artificial.

In *Philosophie Zoologique* (1809), Lamarck first explicitly formulated his ideas as to the transmutation of species, tho he had outlined them as early as 1801. In this memorable publication not only did he state his belief more explicitly and in fuller detail than the idea had been expressed by any predecessor, but he took another long forward step, carrying him far beyond all his forerunners except Darwin, in that he made an attempt to explain the way in which the transmutation of species had been brought about.

The changes have been wrought, he said, through the unceasing efforts of each organism to meet the needs imposed upon it by its environment. Constant striving means the constant use of certain organs, and such use

leads to the development of those organs. Thus a bird running by the sea-shore is constantly tempted to wade deeper and deeper in pursuit of food; its incessant efforts tend to develop its legs, in accordance with the observed principle that the use of any organ tends to strengthen and develop it. But such slightly increased development of the legs is transmitted to the offspring of the bird, which in turn develops its already improved legs by its individual efforts, and transmits the improved tendency. Generation after generation this is repeated, until the sum of the infinitesimal variations, all in the same direction, results in the production of the long-legged wading-bird.

In a similar way, through individual effort and transmitted tendency, all the diversified organs of all creatures have been developed—the fin of the fish, the wing of the bird, the hand of man; nay, more, the fish itself, the bird, the man, even. Collectively the organs make up the entire organism; and what is true of the individual organs must be true also of their *ensemble*, the living being.

Whatever might be thought of Lamarck's explanation of the cause of transmutation—which really was that already suggested by Erasmus Darwin—the idea of the evolution for which he contended was but the logical extension of the conception that American animals are the modified and degenerated descendants of European animals.

But people as a rule are little prone to follow ideas to their logical conclusions, and in this case the conclusions were so utterly opposed to the proximal bearings of the idea that the whole thinking world repudiated them with acclaim. The very persons who had most eagerly accepted the idea of transmutation of European

species into American species, and similar limited variations through changed environment, because of the relief thus given the otherwise overcrowded Ark, were now foremost in denouncing such an extension of the doctrine of transmutation as Lamarck proposed.

And, for that matter, the leaders of the scientific world were equally antagonistic to the Lamarckian hypothesis. Cuvier in particular, once the pupil of Lamarck, but now his colleague, and in authority more than his peer, stood out against the transmutation doctrine with all his force. He argued for the absolute fixity of species, bringing to bear the resources of a mind which, as a mere repository of facts, perhaps never was excelled. As a final and tangible proof of his position, he brought forward the bodies of ibises that had been embalmed by the ancient Egyptians, and showed by comparison that these do not differ in the slightest particular from the ibises that visit the Nile today. Lamarck replied that this proved nothing except that the ibis had become perfectly adapted to its Egyptian surroundings in an early day, historically speaking, and that the climatic and other conditions of the Nile Valley had not since then changed. His theory, he alleged, provided for the stability of species under fixed conditions quite as well as for transmutation under varying conditions.

But, needless to say, the popular verdict lay with Cuvier; talent won for the time against genius, and Lamarck was looked upon as an impious visionary. His faith never wavered, however. He believed that he had gained a true insight into the processes of animate nature, and he reiterated his hypotheses over and over, particularly in the introduction to his *Histoire naturelle des Animaux sans Vertèbres*, in 1815, and in his *Système des Connaissances positives de l' Homme*, in 1820. He lived

on till 1829, respected as a naturalist, but almost unrecognized as a prophet.

While the names of Darwin and Goethe, and in particular that of Lamarck, must always stand out in high relief in this generation as the exponents of the idea of transmutation of species, there are a few others which must not be altogether overlooked in this connection. Of these the most conspicuous is that of Gottfried Reinhold Treviranus, a German naturalist physician, professor of mathematics in the lyceum at Bremen.

It was an interesting coincidence that Treviranus should have published the first volume of his *Biologie, oder Philosophie der lebenden Natur*, in which his views on the transmutation of species were expounded, in 1802, the same twelvemonth in which Lamarck's first exposition of the same doctrine appeared in his *Recherches sur l'Organisation des Corps Vivants*. It is singular, too, that Lamarck, in his *Hydrogéologie* of the same date, should independently have suggested "biology" as an appropriate word to express the general science of living things. It is significant of the tendency of thought of the time that the need of such a unifying word should have presented itself simultaneously to independent thinkers in different countries.

That same memorable year, Lorenz Oken, another philosophical naturalist, professor in the University of Zurich, published the preliminary outlines of his *Philosophie der Natur*, which, as developed through later publications, outlined a theory of spontaneous generation and of evolution of species. Thus it appears that this idea was germinating in the minds of several of the ablest men of the time during the first decade of the century. But the singular result of their various explanations was to give sudden check to that undercurrent

of thought which for some time had been setting toward this conception.

As soon as it was made clear whither the concession that animals may be changed by their environment must logically trend, the recoil from the idea was instantaneous and fervid. Then for a generation Cuvier was almost absolutely dominant, and his verdict was generally considered final.

There was, indeed, one naturalist of authority in France who had the hardihood to stand out against Cuvier and his school, and who was in a position to gain a hearing, tho by no means to divide the following. This was Étienne Geoffroy Saint-Hilaire, the famous author of the *Philosophie Anatomique*, and for many years the colleague of Lamarck at the Jardin des Plantes. Like Goethe, Geoffroy was preeminently an anatomist, and, like the great German, he had early been impressed with the resemblances between the analogous organs of different classes of beings. He conceived the idea that an absolute unity of type prevails throughout organic nature as regards each set of organs. Out of this idea grew his gradually formed belief that similarity of structure might imply identity of origin—that, in short, one species of animal might have developed from another.

Geoffroy's grasp of this idea of transmutation was by no means so complete as that of Lamarck, and he seems never to have fully determined in his own mind just what might be the limits of such development of species. Certainly he nowhere includes all organic creatures in one line of descent, as Lamarck had done; nevertheless he held tenaciously to the truth as he saw it, in open opposition to Cuvier, with whom he held a memorable debate at the Academy of Sciences in 1830—the debate

which so aroused the interest and enthusiasm of Goethe, but which, in the opinion of nearly every one else, resulted in crushing defeat for Geoffroy, and brilliant, seemingly final, victory for the advocate of special creation and the fixity of species.

With that, all ardent controversy over the subject seemed to end, and for just a quarter of a century to come there was published but a single argument for transmutation of species which attracted any general attention whatever. This oasis in a desert generation was a little book called *Vestiges of the Natural History of Creation*, which appeared anonymously in England in 1844, and which passed through numerous editions, and was the subject of no end of abusive and derisive comment. The authorship of this book remained for forty years a secret, but it is now conceded to have been the work of Robert Chambers, the well-known English author and publisher.

The book itself is remarkable as being an avowed and unequivocal exposition of a general doctrine of evolution, its view being as radical and comprehensive as that of Lamarck himself. But it was a *résumé* of earlier efforts rather than a new departure, to say nothing of its technical shortcomings, and, while it aroused bitter animadversions, and cannot have been without effect in creating an undercurrent of thought in opposition to the main trend of opinion of the time, it can hardly be said to have done more than that. Indeed, some critics have denied it even this merit. After its publication, as before, the conception of transmutation of species remained in the popular estimation, both lay and scientific, an almost forgotten "heresy."

It is true that here and there a scientist of greater or less repute—as Von Buch, Meckel, and Von Baer in

Germany, Bory Saint-Vincent in France, Wells, Grant, and Matthew in England, and Leidy in America—had expressed more or less tentative dissent from the doctrine of special creation and immutability of species, but their unaggressive suggestions, usually put forward in obscure publications, were utterly overlooked and ignored. And so, despite the scientific advances along many lines at the middle of the century, the idea of the transmutability of organic races had no such prominence, either in scientific or unscientific circles, as it had acquired fifty years before. Special creation held the day, apparently unchallenged and unopposed.

But even at this time the fancied security of the special-creation hypothesis was by no means real. Tho it seemed so invincible, its real position was that of an apparently impregnable fortress beneath which, all unbeknown to the garrison, a powder-mine has been dug and lies ready for explosion. For already there existed in the secluded work-room of an English naturalist, a manuscript volume and a portfolio of notes which might have sufficed, if given publicity, to shatter the entire structure of the special-creation hypothesis. The naturalist who, by dint of long and patient effort, had constructed this powder-mine of facts was Charles Robert Darwin, grandson of the author of *Zoönomia*.

As long ago as July 1, 1837, young Darwin, then twenty-eight years of age, had opened a private journal, in which he purposed to record all known facts bearing on the moot point of the doctrine of transmutation of species. Four or five years earlier, during the course of that famous trip around the world with Admiral Fitzroy, as naturalist to the *Beagle*, Darwin had made the personal observations which first tended to shake his belief in the fixity of species.



In South America, in the Pampean formation, he had discovered "great fossil animals covered with armor like that on the existing armadillos," and had been struck with this similarity of type between ancient and existing faunas of the same region. He was also greatly impressed by the manner in which closely related species of animals were observed to replace one another as he proceeded southward over the continent; and "by the South American character of most of the productions of the Galápagos Archipelago, and more especially by the manner in which they differ slightly on each island of the group, none of the islands appearing to be very ancient in a geological sense."

At first the full force of these observations did not strike him; for, under sway of Lyell's geological conceptions, he tentatively explained the relative absence of life on one of the Galápagos Islands by suggesting that perhaps no species had been created since that island arose. But gradually it dawned upon him that such facts as he had observed "could only be explained on the supposition that species gradually become modified." From then on, as he afterwards asserted, the subject haunted him; hence the journal of 1837.

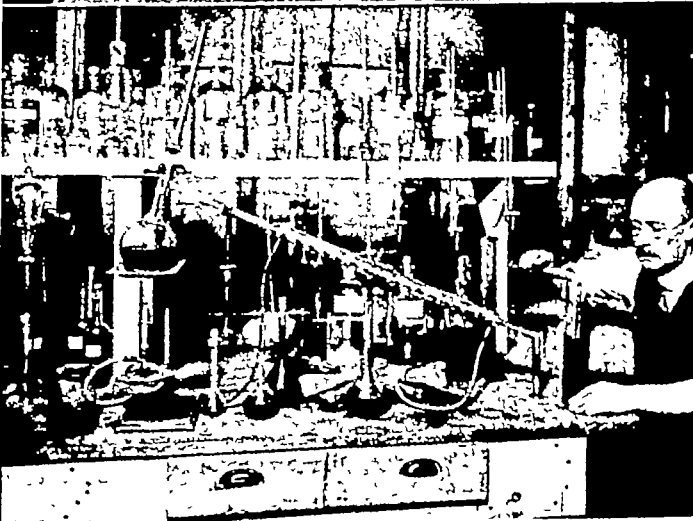
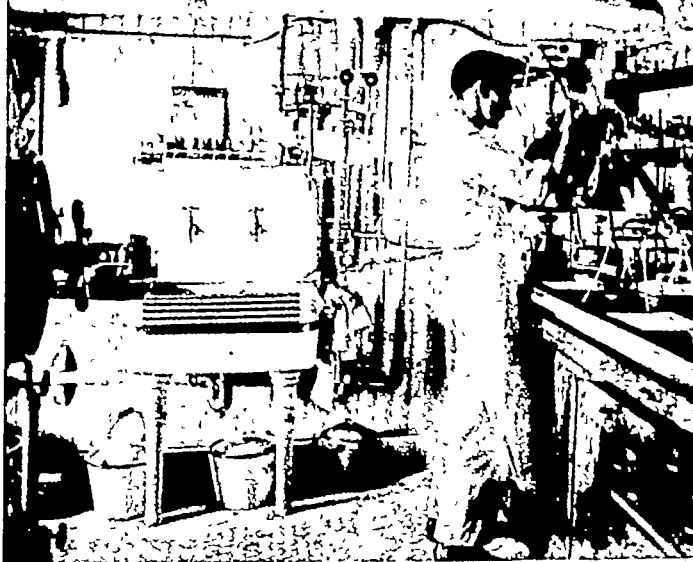
It will thus be seen that the idea of the variability of species came to Charles Darwin as an inference from personal observations in the field, not as a thought borrowed from books. He had, of course, read the works of his grandfather much earlier in life, but the arguments of the *Zoönomia* and *Temple of Nature* had not served in the least to weaken his acceptance of the current belief in fixity of species. Nor had he been more impressed with the doctrine of Lamarck, so closely similar to that of his grandfather. Indeed, even after his South American experience had aroused him to a new

point of view he was still unable to see anything of value in these earlier attempts at an explanation of the variation of species.

In opening his journal, therefore, he had no preconceived notion of upholding the views of these or any other makers of hypotheses, nor at the time had he formulated any hypothesis of his own. His mind was open and receptive; he was eager only for facts which might lead him to an understanding of a problem which seemed utterly obscure. It was something to feel sure that species have varied; but how have such variations been brought about?

It was not long before Darwin found a clue which he thought might lead to the answer he sought. In casting about for facts he had soon discovered that the most available field for observation lay among domesticated animals, whose numerous variations within specific lines are familiar to every one. Thus under domestication creatures so tangibly different as a mastiff and a terrier have sprung from a common stock. So have the Shetland pony, the thoroughbred, and the draft horse. In short, there is no domesticated animal that has not developed varieties deviating more or less widely from the parent stock. Now how has this been accomplished? Why, clearly, by the preservation, through selective breeding, of seemingly accidental variations. Thus one horseman, by constantly selecting animals that "chance" to have the right build and stamina, finally develops a race of running horses; while another horseman, by selecting a different series of progenitors, has developed a race of slow, heavy draft animals.

So far so good; the preservation of "accidental" variations through selective breeding is plainly a means by which races may be developed that are very different



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from their original parent form. But this is under man's supervision and direction. By what process could such selection be brought about among creatures in a state of nature? Here surely was a puzzle, and one that must be solved before another step could be taken in this direction.

The key to the solution of this puzzle came into Darwin's mind through a chance reading of the famous essay on "Population" which Thomas Robert Malthus had published almost half a century before. This essay, expositing ideas by no means exclusively original with Malthus, emphasizes the fact that organisms tend to increase at a geometrical ratio through successive generations, and hence would overpopulate the earth if not somehow kept in check. Cogitating this thought, Darwin gained a new insight into the processes of nature. He saw that in virtue of this tendency of each race of beings to overpopulate the earth, the entire organic world, animal and vegetable, must be in a state of perpetual carnage and strife, individual against individual, fighting for sustenance and life.

That idea fully imagined, it becomes plain that a selective influence is all the time at work in nature, since only a few individuals, relatively, of each generation can come to maturity, and these few must, naturally, be those best fitted to battle with the particular circumstances in the midst of which they are placed. In other words, the individuals best adapted to their surroundings will, on the average, be those that grow to maturity and produce offspring. To these offspring will be transmitted the favorable peculiarities. Thus these peculiarities will become permanent, and nature will have accomplished precisely what the human breeder is seen to accomplish.

Grant that organisms in a state of nature vary, how-

ever slightly, one from another (which is indubitable), and that such variations will be transmitted by a parent to its offspring (which no one then doubted); grant, further, that there is incessant strife among the various organisms, so that only a small proportion can come to maturity—grant these things, said Darwin, and we have an explanation of the preservation of variations which leads on to the transmutation of species themselves.

This wonderful coign of vantage Darwin had reached by 1839. Here was the full outline of his theory; here were the ideas which afterwards came to be embalmed in familiar speech in the phrases "spontaneous variation," and the "survival of the fittest," through "natural selection."

After such a discovery any ordinary man would at once have run through the streets of science, so to speak, screaming "Eureka!" Not so Darwin. He placed the manuscript outline of his theory in his portfolio, and went on gathering facts bearing on his discovery. In 1844 he made an abstract in a manuscript book of the mass of facts by that time accumulated. He showed it to his friend Hooker, made careful provision for its publication in the event of his sudden death, then stored it away in his desk, and went ahead with the gathering of more data. This was the unexploded powder mine.

Twelve years more elapsed; years during which the silent worker gathered a prodigious mass of facts, answered a multitude of objections that arose in his own mind, vastly fortified his theory. All this time the toiler was an invalid, never knowing a day free from illness and discomfort, obliged to husband his strength, never able to work more than an hour and a half at a stretch; yet he accomplished what would have been vast achievements for half a dozen men of robust health.

Two friends among the eminent scientists of the day knew of his labors—Sir Joseph Hooker, the botanist, and Sir Charles Lyell, the geologist. Gradually Hooker had come to be more than half a convert to Darwin's views. Lyell was still skeptical, yet he urged Darwin to publish his theory without further delay, lest he be forestalled. At last the patient worker decided to comply with this advice, and in 1856 he set to work to make another and fuller abstract of the mass of data he had gathered.

And then a strange thing happened. After Darwin had been at work on his "abstract" about two years, but before he had published a line of it, there came to him one day a paper in manuscript, sent for his approval by a naturalist friend, named Alfred Russel Wallace, who had been for some time at work in the East India Archipelago. He read the paper, and, to his amazement, found that it contained an outline of the same theory of "natural selection" which he himself had originated and for twenty years had worked upon. Working independently, on opposite sides of the globe, Darwin and Wallace had hit upon the same explanation of the cause of transmutation of species. "Were Wallace's paper an abstract of my unpublished manuscript of 1844," said Darwin, "it could not better express my ideas."

Here was a dilemma. To publish this paper with no word from Darwin would give Wallace priority, and wrest from Darwin the credit of a discovery which he had made years before his co-discoverer entered the field. Yet, on the other hand, could Darwin honorably do otherwise than publish his friend's paper and himself remain silent? It was a complication well calculated to try a man's soul. Darwin's was equal to the test.

Keenly alive to the delicacy of the position, he placed



THOMAS HENRY HUXLEY

the whole matter before his friends Hooker and Lyell, and left the decision as to a course of action absolutely to them. Needless to say, these great men did the one thing which insured full justice to all concerned. They counseled a joint publication, to include on the one hand Wallace's paper, and on the other an abstract of Darwin's ideas, in the exact form in which it had been outlined by the author in a letter to Asa Gray in the previous year—an abstract which was in Gray's hands before Wallace's paper was in existence.

This joint production, together with a full statement of the facts of the case, was presented to the Linnæan Society of London by Hooker and Lyell on the evening of July 1, 1858, this being, by an odd coincidence, the twenty-first anniversary of the day on which Darwin had opened his journal to collect facts bearing on the "species question." Not often before in the history of science has it happened that a great theory has been nurtured in its author's brain through infancy and adolescence to its full legal majority before being sent out into the world.

Thus the fuse that led to the great powder-mine had been lighted. The explosion itself came more than a year later, in November, 1859, when Darwin, after thirteen months of further effort, completed the outline of his theory, which was at first begun as an abstract for the Linnæan Society, but which grew to the size of an independent volume despite his efforts at condensation, and which was given that ever-to-be-famous title, *The Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life*.

And what an explosion it was! The joint paper of 1858 had made a momentary flare, causing the hearers, as Hooker said, to "speak of it with bated breath," but

beyond that it made no sensation. What the result was when the *Origin* itself appeared, no one of our fathers' generation need be told. The rumble and roar that it made in the intellectual world have not yet altogether ceased to echo after seventy-five years of reverberation.

To the *Origin of Species*, then, and to its author, Charles Darwin, must always be ascribed chief credit for that vast revolution in the fundamental beliefs of our race which has come about since 1859.

But it must not be overlooked that no such sudden metamorphosis could have been effected had it not been for the aid of a few notable lieutenants, who rallied to the standard of the leader immediately after the publication of the *Origin*. Darwin had all along felt the utmost confidence in the ultimate triumph of his ideas "Our posterity," he declared in a letter to Hooker, "will marvel as much about the current belief [in special creation] as we do about fossil shells having been thought to be created as we now see them." But he fully realized that for the present success of his theory of transmutation the championship of a few leaders of science was all-essential. He felt that if he could make converts of Hooker and Lyell and of Thomas Henry Huxley at once, all would be well.

His success in this regard, as in others, exceeded his expectations. Hooker was an ardent disciple from reading the proof-sheets before the book was published; Lyell renounced his former beliefs and fell into line a few months later; while Huxley, so soon as he had mastered the central idea of natural selection, marveled that so simple yet all-potent a thought had escaped him so long, and then rushed eagerly into the fray, wielding the keenest dialectic blade that was drawn during the entire controversy.

Then, too, unexpected recruits were found in Sir John Lubbock and John Tyndall, who carried the war eagerly into their respective territories; while Herbert Spencer, who had advocated a doctrine of transmutation on philosophic grounds some years before Darwin published the key to the mystery—and who himself had barely escaped independent discovery of that key—lent his masterful influence to the cause. In America, the famous botanist Asa Gray, who had long been a correspondent of Darwin's, but whose advocacy of the new theory had not been anticipated, became an ardent propagandist; while in Germany Ernst Heinrich Haeckel the youthful but already noted zoologist, took up the fight with equal enthusiasm.

Against these few doughty champions—with here and there another of less general renown—was arrayed, at the outset, practically all Christendom. The interest of the question came home to every person of intelligence, whatever his calling, and the more deeply as it became more and more clear how far-reaching are the real bearings of the doctrine of natural selection.

Soon it was seen that should the doctrine of the survival of the favored races through the struggle for existence win, there must come with it as radical a change in man's estimate of his own position as had come in the day when, through the efforts of Copernicus and Galileo, the world was dethroned from its supposed central position in the universe. The whole conservative majority of mankind recoiled from this necessity with horror. And this conservative majority included not laymen merely, but a vast preponderance of the leaders of science also.

With the open-minded minority, on the other hand, the theory of natural selection made its way by leaps

and bounds. Its delightful simplicity—which at first sight made it seem neither new nor important—coupled with the marvelous comprehensiveness of its implications, gave it a hold on the imagination, and secured it a hearing where other theories of transmutation of species had been utterly scorned. Men who had found Lamarck's conception of change through voluntary effort ridiculous, and the vaporings of the *Vestiges* altogether despicable, men whose scientific cautions held them back from Spencer's deductive argument, took eager hold of that tangible, ever-present principle of natural selection, and were led on and on to its goal. Hour by hour the attitude of the thinking world toward this new principle changed; never before was so great a revolution wrought so suddenly.

Nor was this merely because "the times were ripe" or "men's minds prepared for evolution." Darwin himself bears witness that this was not altogether so. All through the years in which he brooded this theory he sounded his scientific friends, and could find among them not one who acknowledged a doctrine of transmutation. The reaction from the standpoint of Lamarck and Erasmus Darwin and Goethe had been complete, and when Charles Darwin avowed his own conviction he expected always to have it met with ridicule or contempt. In 1857 there was but one man speaking with any large degree of authority in the world who openly avowed a belief in transmutation of species—that man being Herbert Spencer. But the *Origin of Species* came, as Huxley has said, like a flash in the darkness, enabling the benighted voyager to see the way. The score of years during which its author had waited and worked had been years well spent. Darwin had become, as he himself says, a veritable Cræsus, "overwhelmed with his

riches in facts"—facts of zoology, of selective artificial breeding, of geographical distribution of animals, of embryology, of paleontology. He had massed his facts about his theory, condensed them and recondensed, until his volume of five hundred pages was an encyclopedia in scope.

During those long years of musing he had thought out almost every conceivable objection to his theory, and in his book every such objection was stated with fullest force and candor, together with such reply as the facts at command might dictate. It was the force of those twenty years of effort of a master mind that made the sudden breach in the breastwork of current thought.

Once this breach was effected, the work of conquest went rapidly on. Day by day squads of the enemy capitulated and struck their arms. By the time another score of years had passed the doctrine of evolution had become the working hypothesis of the scientific world. The revolution had been effected.

And from amid the wreckage of opinion and belief stands forth the figure of Charles Darwin, calm, imperturbable, serene; scatheless to ridicule, contumely, abuse; unspoiled by ultimate success; unsullied alike by the strife and the victory—take him for all in all, for character, for intellect, for what he was, and what he did, perhaps the most Socratic figure of his century. When, in 1882, he died, friend and foe alike conceded that one of the greatest sons of men had rested from his labors, and all the world felt it fitting that the remains of Charles Darwin should be entombed in Westminster Abbey, close beside the honored grave of Isaac Newton.

Dear my dear friend.

My dear friend.

I'm sending my excuses for the short life, only got you to 2 letters last night the postal boat of 10 I found one long letter, and intended before my boat to answer you this very afternoon as soon as I'd seen it a little. That by saying back to you, in the evening with my book and the accompanying of the minute.

The pretty ones and day in behalf of the B.M. what little I have - had his always found the very greatest attention & did know - that I was away from Paris to London to write an article in a review about French affairs - so that when I went to the Bibliothèque de la rue I could only get a book at a time, and no sight of a catalogue, but then I didn't go after being disappointed with the place, and asking it as a thing without any recommendation.

By this morning can be afforded by letter I should like to send better if it is some years old now) and if by word of mouth, for I have not seen it before at H.C. or elsewhere but the end of the world.

And don't be saying back to me, my dear old fellow, for not writing, and I thought I might put it off till day afterwards (which I have now done).

Yours always truly my dear friend

W. M. Thackeray.

WILLIAM MAKEPEACE THACKERAY.

Date, 1831

British Museum, Additional MS 34,327



487
Cambridge Feb 1
1864

Dear Sir,

I have this morning
had the pleasure of receiving
the copy of your new and
very well edited of Chan-
cer's "Legends of Good
Women", which you were
so kind as to send me
and for which I beg you

HENRY WADSWORTH LONGFELLOW.

Date, 1864

British Museum, Add. MS 33,064



Калъ зърне, калъ стане, бжеа възх!..

Будь же, когда он, добро-добро тому кажет, и
привади нас отсюда, вонючую. Это тоже кажет
он. Но прости, отсюда отсюда, уйдя в море.

Keep approx. 1000 chinks than pyth.

Ministri gade, anggi gajagant comela sont; by
memari koranant upant gane dhar. A rasy, sa.
Tatmad ti yint; a ti waditi ti jhama sa, shomad

Let's agree. Let's stand man high.

[illegible]

Rolls exposed, left chamber then open.

et de connaître le monde, par exemple. Sans
aucune charge, cependant, l'état n'a pas
à payer les dépenses de voyage, ni les dépenses
de logement, ni les dépenses de nourriture,
ni les dépenses de transport.

Reds 6: green, Red's clutch 11 new eggs.

Pomacanthus niger niger regis. *Pomacanthus*
leucostriatus niger. *Pomacanthus* niger. - 1870 pp.
Pomacanthus niger regis. *Pomacanthus* niger.
Pomacanthus niger regis. *Pomacanthus* niger.

IVAN SERGEEVICH TURGENIEFF.

March 1861, April 1861



on sheet at the back of page.

Aspects of Culture.

ii
+ Address read before the Phi Beta
Kappa Society, Harvard University,
July 1867.
~~After Professor of Literature,~~

We meet today.

Under happy omens to our
ancient society, to the Common-
wealth of letters, to the Country,
to mankind to good Citizens
But shares the wonderful
prosperity of the Federal Union
The heart still beats with the
public pulse & joy, that the
Country has withstood the
rude trial which threatened

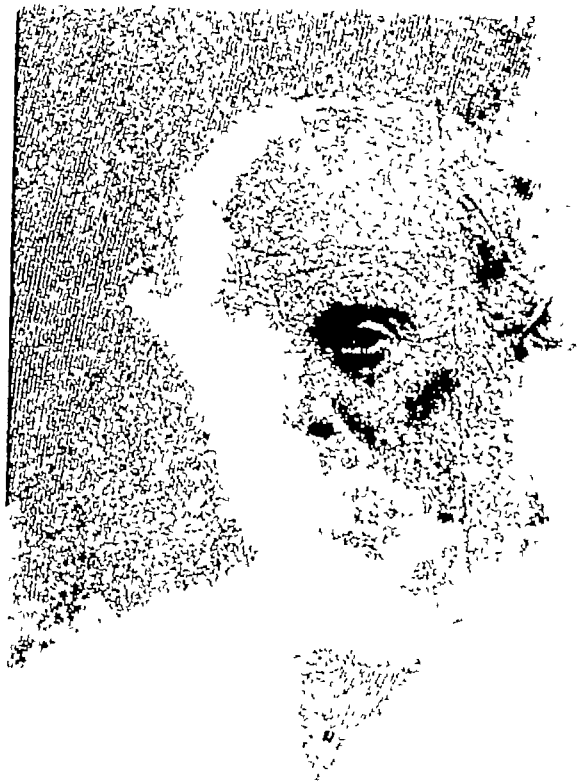
RALPH WALDO EMERSON

June, 1867

Forster Museum, A 64 MS 23.5.13







LORD LISTER





LUTHER BURBANK

V

PLANTS IN THE SERVICE OF MAN

SOMETHING has been said about the great basic discoveries that were made before the dawn of civilization. Among the very greatest of all the early achievements was the cultivation of plants. Long before what we term the dawn of civilization, a whole series of vegetable forms had been brought into the service of man, and had contributed to human progress in the most significant manner.

More than that: The original or native forms of vegetation had been modified by selective plant breeding, carried out no doubt by hundreds of generations of men, until it was impossible, even in the classical period of history, to discover what the original forms had been from which the familiar types of cereals, vegetables, and fruits had been developed. There is not even a hint or reminiscence as to the actual name, much less the nationality, of any one of the scientific heroes who had a significant share in this development.

The truth is, probably, that for the most part there was no individual to be greatly credited. In all probability, the art of selection, which in modern times we speak of as plant breeding, was carried out generation after generation by practical tillers of the soil, who merely exercised common sense based on the observed fact that individual plants differ, and that the seeds of the plant tend to transmit the peculiarities of the parent form. The full force of the clear-cut appreciation of the power

of heredity is conveyed in such a phrase as the Biblical quotation: "Do men gather grapes of thorns or figs of thistles?"

It was early discovered, however, that plants may also undergo modifications that are not transmissible by seeds. This proved to be true of orchard fruits in particular. In these cases, however, it was found possible to propagate and spread useful varieties by the process of grafting. This process was perfectly understood by the nations of antiquity, and was utilized in the distribution of fruit trees.

It is rather startling to note that, until the discovery by Mendel of the principle of segregation of character in heredity, there was no essential change in the principles of plant breeding understood and utilized by prehistoric man and by his descendants even of the nineteenth century. Only since the year 1900, when Mendel's forgotten experiments were ferreted out of the obscure publications in which they had been buried, has it been possible to make predictable changes in plant forms by another method than what might be called trial and error; and even now the principle of selection of the best forms, among many divergent plants, is by no means ignored. In other words, the old familiar principle of heredity — according to which, in general terms, like produces like — is still fundamental and perennially operative.

It was through utilization of this principle that our remote forebears developed the varied forms of plant life that so influence the progress of civilization. Since we cannot know the names of these remote plant developers, and can only infer in general terms when and where they worked, our best resource will be to consider the work of one of the most famous of modern plant



breeders, Luther Burbank, whose work, prior to the re-discovery of Mendel's principle, was carried out along ancestral lines, so to speak, and may be taken as typifying the antique method.

A few illustrations of specific achievements in plant breeding will serve to reveal the method, and may be taken as typical of what has been done, and may be done, with many other types of cultivated plants.

The work of the plant breeder, like that of the developer of domesticated animals, gives a practical and unmistakable demonstration of the operation of what, in the natural world, we describe as the evolutionary process. Artificial selection, on the part of man, takes the place of "natural selection" to a certain extent, but the underlying principles are not modified; and man at best takes his lessons from nature. Here as elsewhere the experimenter utilizes natural forces. He modifies, but he cannot create.

The phrase, "new creations in plant life," which Luther Burbank made famous, is perhaps a justifiable exaggeration, but it will not bear too critical examination. Nevertheless, it is not to be doubted that, granted time enough, the observed modifications would produce such cumulative changes as to make the new forms specifically different from the parent forms.

Even as the case stands, the familiar types of cultivated vegetables and fruits are so tremendously changed, and from the human standpoint so greatly improved, that their origin is in doubt. In most cases, however, there are wild forms, botanically allied, which may be accepted as at least closely akin to the direct ancestors of the existing forms. Our present concern, however, is with the recent modification of varieties of plants that were already highly developed.

We naturally think of Luther Burbank as a Californian; but in reality the celebrated plant experimenter was born and reared in Massachusetts. The little town of Chester was his birthplace, and he grew to maturity on his father's farm in daily contact with nature in her somewhat primitive aspects.

Mr. Burbank himself called attention, not without amusement, to the fact that he was his father's thirteenth child; and he used this fact to give whimsical support to his own familiar method of "quantity production" in plant breeding, pointing out that no one of the first dozen children of his fraternity showed any particular propensity to devote attention to plant development, and drawing therefrom the serious conclusion that the full potentialities of any hereditary strain can not be realized unless the old-fashioned custom of having large families is practised.

It is a moral worthy of sober attention in these days when the Colonial stock, of which Mr. Burbank himself furnishes a rather typical example, is relatively dwindling, to the detriment—at least so some of us think—of our civilization.

Luther Burbank was a rather frail child, tho not without abounding physical vigor. He was of a thoughtful, studious bent of mind, with an inherent love of flowers and plants that manifested itself at a very early age, and with an almost equally striking fondness for mechanics. It is recorded that one of his most fondly prized toys in infancy was a specimen of spineless cactus, and that the possession of a flower would almost always quiet him and give him, seemingly, greater pleasure than he derived from any toy.

His inventive bent manifested itself very early, and led him to the devising of many mechanisms, including

a home-made steam-engine, which he used to propel a boat, producing thus a prototype of the modern motor boat half a century before the craft gained popularity.

The most conspicuous application of young Burbank's mechanical genius, however, was made in a factory where he went to work just as he was verging on maturity. This was a labor-saving device of such usefulness that it enabled him to multiply the efficiency of his work tenfold, so that his earnings, which at first had amounted to only fifty cents a day, quickly mounted to a really respectable figure. He might have remained indefinitely in the factory with the assurance of a good salary; but the confinement proved unhealthful, and he soon returned to the fields, never thereafter to leave them.

The inventive genius hitherto applied to mechanical apparatus was now transferred to the living plant, and from the outset young Burbank began experimenting along new lines even in carrying out the most commonplace work of the gardener. For instance, he found a way to force the development of his sweet corn by sprouting the seed in a hotbed and dropping the young plants into hills in the open as if they were mere seed kernels; and he performed a great variety of interesting experiments in the cross-fertilization of different races of beans, of sweet corn, and of various other garden products.

Nothing strikingly notable came of this work, however, until an occasion when the experimenter discovered a seed ball on the vine of an Early Rose potato; saved the twenty-three seeds that the ball contained, and grew from each of them a hill of potatoes next season. The twenty-three hills were in a single row, and were given precisely the same attention, yet each produced a quite different type of tuber; and one of the hills revealed a



GIANT CACTUS

large cluster of potatoes of such exceptional size and smoothness of contour and quality of flesh as to be very notable.

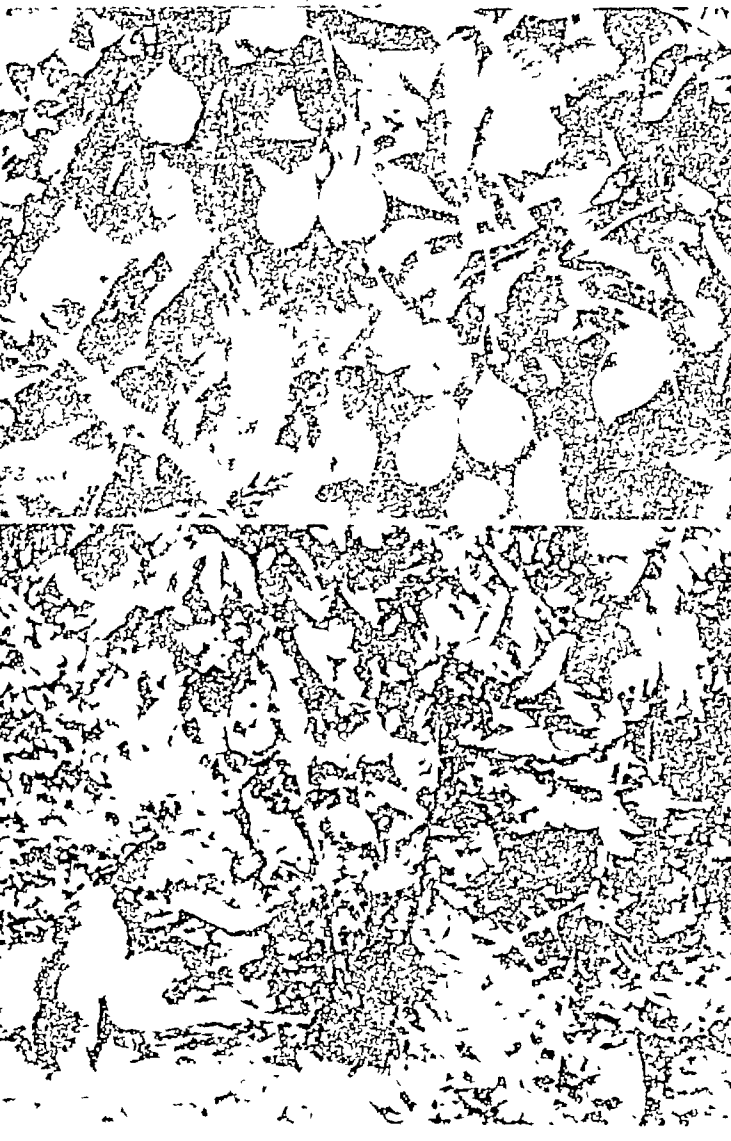
This was the potato which the young experimenter sold next season to a practical gardener, who gave it the name of the Burbank potato.

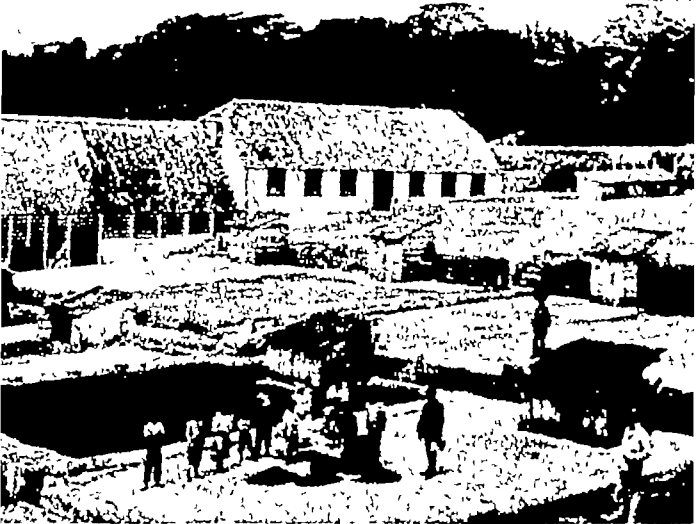
It was estimated several years ago by the authorities of the Department of Agriculture at Washington that more than seventeen million dollars' worth of Burbank potatoes had been raised in the United States since the variety was introduced. The producer himself received only one hundred and fifty dollars for his prize. The money sufficed, however, to pay his fare across the continent, and enabled him to carry out his ambition to migrate to a climate better suited to the purposes of the plant developer—for he had long since determined to give his life to this work.

Arriving in California, Mr. Burbank selected Santa Rosa as his residence, and this continued to be the seat of his activities to the end of his life.

The migration was made in 1875. At that time the potentialities of California as a fruit-growing State were not very fully realized, and it was by no means easy for a young man without capital to establish himself in the practical business of a nurseryman, which was Luther Burbank's immediate ambition. Before he could carry out this ambition, it was necessary to serve an apprenticeship of two or three years, during which he turned his hand to any work which presented itself. He developed skill as a carpenter, and he continued to earn a living at that trade for some time after he had established a nursery by way of avocation.

Those were trying years; but Yankee thrift, energy, and perseverance finally prevailed over all obstacles and





PICKING PEPPERS, SINGAPORE — DRYING
ALLSPICE, JAMAICA

within four or five years after coming to California Mr. Burbank found himself in possession of a commercial nursery that netted him an annual income of about ten thousand dollars. His orchard products were mostly of standard varieties, but he had applied to them from the outset the selective skill that was to make him famous, and he had gained for his seedlings a reputation for reliability that caused them to be bought by would-be orchardists throughout the fruit-growing region.

Such commercial success was gratifying, but Burbank regarded it as only a stepping-stone. Even while his chief time was necessarily given over to the practical duties of the nurseryman, he found opportunity to make numberless experiments in hybridization and selection among the various plants in his nursery; and so soon as his financial affairs gave the least promise of security, he cast about for a piece of land on which he could establish an experiment garden to be devoted exclusively to the production of new and improved varieties of plants of every type.

He found four acres that could be made available by proper drainage and fertilization, in the town of Santa Rosa, and there he established the garden that was soon to be famous as the seat of the most remarkable series of plant experiments ever carried out in our country. A little later he purchased a tract of eighteen acres at Sebastopol, seven miles away, where the topographical and climatic conditions were slightly different. There his main experiment orchards were established, and opportunity was afforded for the carrying out of the idea of "quantity production" more effectively than was possible in the restricted area of the Santa Rosa garden.

From that day forth, Burbank conducted his experiments on these two plots of land, aggregating about

twenty-two acres. Within this relatively small area more than a hundred thousand distinct experiments were carried out, involving five or six thousand species of plants, and numberless varieties; the original seeds or stock or roots were sent to Burbank from all parts of the world.

Probably there is no other similar area of the earth's surface that has seen a corresponding variety of vegetable products in the same time; certainly there is no other that in our day has produced such a galaxy of new and wonderful plant products as have grown in the experiment gardens at Santa Rosa and Sebastopol.

The fundamental principles of plant development through which Burbank thought to develop new and improved varieties were not in themselves novel or revolutionary. They consisted essentially in the careful selection among a mass of plants of any individual that showed exceptional qualities of a desirable type; the saving of seed of this exceptional individual, and the carrying out of the same process of selection among the progeny through successive generations.

Couple this method of selection and so-called line breeding with the method of cross-pollenizing different varieties or species, to produce hybrid forms showing a tendency to greater variation or to the accentuation of desired characters, and we have in outline the fundamental principles of plant breeding as known to horticulturists for generations, and as applied by Burbank from the outset of his career. But there were sundry highly essential details of modification that were introduced by the Santa Rosa experimenter, as will appear presently.

Moreover, even in the application of the old familiar method, Burbank was able from the outset to gain exceptional results because of certain inherent qualities that





RED PEPPERS IN JAPAN: GATHERING
AND SORTING

peculiarly fitted him for the work. Among these qualities was his exceedingly acute vision, a remarkable color sense, and almost abnormally developed senses of smell and taste. Artists who tested his eyes declared that he could readily detect gradations of color that to the ordinary eye showed no differentiation whatever; and it was a matter of hourly demonstration that he could ferret out an individual flower having any infinitesimally modified odor in the midst of a bed of thousands of such plants, almost as a hunting dog detects the location of a grouse or partridge under cover.

Similarly his exquisitely refined sense of taste guided him in selecting among thousands of individual plums or cherries or grapes or apples or berries the one individual specimen that had the most delectable flavor or that showed a minute modification of flavor in the direction in which he was endeavoring to modify the variety.

This almost preternatural endowment of special senses was supplemented by a knowledge of the coordination of parts—say between the stem or leaf and the future fruit of a plant—that was so penetrating and mystifying as to seem intuitional and to suggest occult powers of divination.

As an instance, you might see Mr. Burbank striding along a row of, let us say, plum seedlings comprising some thousands of plants perhaps a foot high. He seems to inspect the little trees but casually, except that now and again he pauses for a moment to indicate with a motion of his hand that this or that plant has particularly attracted his attention. A helper, or more likely two helpers—for one can scarcely keep up with the energetic leader—will be at hand to note the signals; and a bit of white cloth will be tied about each successively selected seedling; or two pieces of cloth, or even three,



CLOVE TREES, AFRICA—NUTMEG TREES,
SINGAPORE

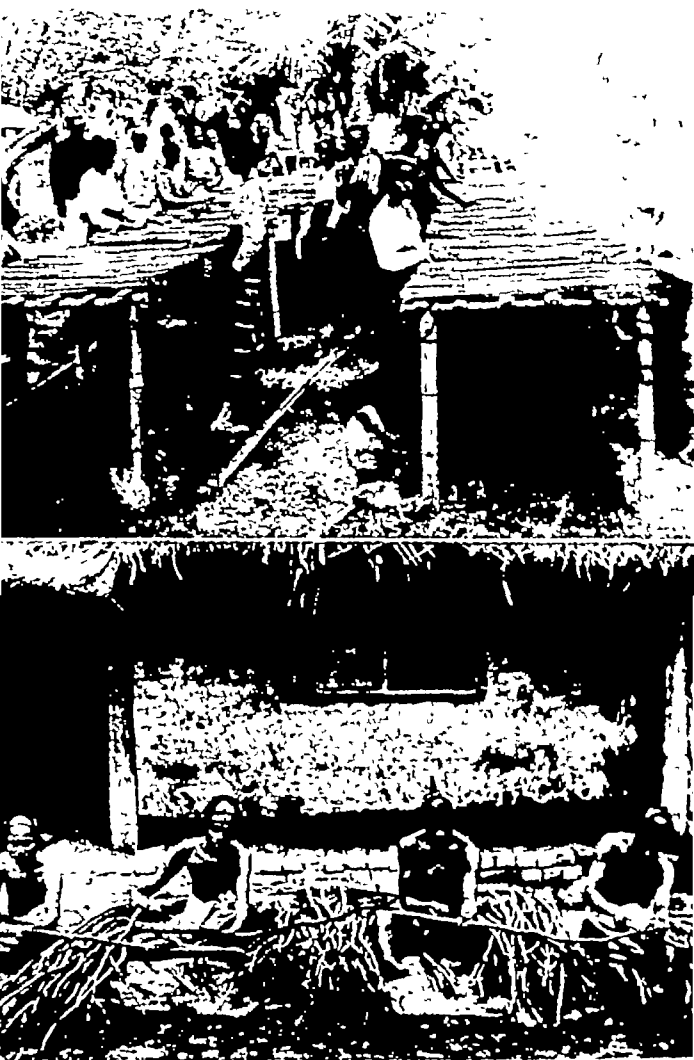
in case an individual has seemed to show quite exceptional promise.

And with that, one stage of the work of selection is finished. Perhaps ten thousand seedlings have been passed in review in a half-hour, and conceivably fifty or a hundred have been selected for preservation. These have shown to the keen scrutiny of the plant experimenter such qualities of stem and bud and leaf as to forecast the type of fruit sought to be developed in this particular experiment.

The entire row of seedlings are the product of hybridizing experiments and antecedent selection extending perhaps through many generations. The seed from which they were grown has been carefully gathered and treasured, and infinite pains have been taken to bring the seedlings, through transplantation and cultivation, to their present stage of development.

Yet now, in a single half-hour, they have been made to run the gantlet of a vision that seems to penetrate to the very heart of their germ-plasm, like an X-ray, and all but a bare half-dozen or so in each thousand have been found wanting. Another hour, and the ten thousand failures—less the half-hundred—will have been uprooted and piled in a heap to be burned like any other mass of rubbish. They had done their best, but their best was not good enough; and the soil that they occupied must be given over to some other line of experiments; for every acre of these gardens must be made to do the work of a score of acres.

Meantime the dozen or score of selected seedlings that remain as the lone survivors here and there in the devastated ranks will be treasured and be given every horticultural attention. At the proper season they will come under the knife of the grafter, who will cut each stem



CINNAMON IN CEYLON

into appropriate sections and graft pieces on the limbs of some sturdy tree of the same species. This is done to hasten their development, for Mr. Burbank has discovered that stems thus grafted will come to bearing much earlier than if left on their original roots. Time is precious, particularly when we are dealing with plants of such slow growth as the fruit trees, and it is obviously worth while to save a year or two, as is thus possible; for at best an experiment in the development of a new type of fruit must be carried out, as a rule, through a good many generations, making significant encroachments on the working life of the plant experimenter himself.

Where such a method as that just outlined is carried out, it is obvious that everything depends upon the skill with which selection is made. A man lacking Mr. Burbank's "intuitional" skill in such a selection would inevitably go wrong. His experiments would come to nothing. He would inadvertently destroy the best and preserve the worst. By no mathematical chance could he select the right dozen or score of individuals among the tens of thousands.

But that Luther Burbank was able to make such selections with a correctness that was little less than weird was demonstrated again and again through tests in which various of the discarded seedlings were preserved and brought to fruitage for comparison with the selected ones of their fraternity.

Always the selected individuals showed more of the quality that was being sought than was shown by the specimens taken from the discard; thus justifying a forecast that was made so readily with such seeming facility as to appear almost necromantic.

In point of fact, the plant experimenter was exercis-

ing no occult powers but only trained senses backed by an amazing fund of practical knowledge. He was looking for stems of a particular size and ruggedness of contour; for leaves that were symmetrical, right-hued, and thrifty; for buds that were plump and fat and of just the right color. But his eye took in the details so quickly and his conclusions were reached with such seemingly automatic precision, that the entire procedure took on a mystifying aspect of wizardry.

With such exhibitions of his skill constantly in evidence, it is not strange that Burbank should have become traditional among his own contemporaries as the "wizard of Santa Rosa"; altho the worker himself always ardently deprecated any such characterization, calling himself a "plant experimenter," and being foremost to affirm that what he accomplished was done by careful study of the laws of heredity, ceaseless scrutiny of the physical qualities of plants in their every aspect, and the definite application of knowledge gained through thousands of antecedent experiments.

The range and scope of these experiments, it may be added, were no less astounding than the manner in which they were carried out. There was scarcely a tribe of plants showing any promise whatever of development of its stock or root or flower or fruit and having the remotest prospect of thriving under the climatic conditions of Santa Rosa and Sebastopol that was not tested by specimens brought from one corner or another of the world—from both hemispheres and from every continent—and set to work in Mr. Burbank's training school.

To give the names of the different species and varieties that have here been modified and improved through selective breeding—quite overlooking the other legions that have proved recalcitrant—would require many pages



So I must be content with the citation of only a few of the more conspicuous examples.

Consider, for example, the orchard fruits. Mr. Burbank produced almost numberless new varieties of apples, pears, peaches, apricots, plums, prunes, cherries, and quinces. He introduced more than sixty new varieties of plums and prunes, combining the strains of ancestors from Europe and Japan with those of our native species, and producing some extraordinary fruits.

Here, for example, are prunes that are not only of gigantic size and borne in profusion, but which have a quality of ripening in midsummer and of developing a greatly increased sugar content. Here are plums that add to their other qualities the capacity to withstand shipment across the continent, or for that matter 'round the world. Here is one plum that looks and tastes like an apple and another that has precisely the quality of a Bartlett pear. And here are plums and prunes that while exteriorly looking like other fine specimens of their kind differ essentially from all others in that you could bite right through them as you bite through a strawberry, because they are stoneless.

And then, most marvelous of all, here is a fruit that had a plum for one of its ancestors, but for another ancestor an apricot; a strange hybrid which, in recognition of its origin, was named the "plumcot" and which constitutes a brand-new type of orchard fruit, the first addition that has been made to the familiar list within historical times, and the only orchard fruit whose origin is definitely known. This one was created at Sebastopol, as the result of a long series of tests in cross-pollenizing the plum and apricot; tests which at first seemed doomed to failure, but which ultimately culminated in the production of a wonderful new fruit.

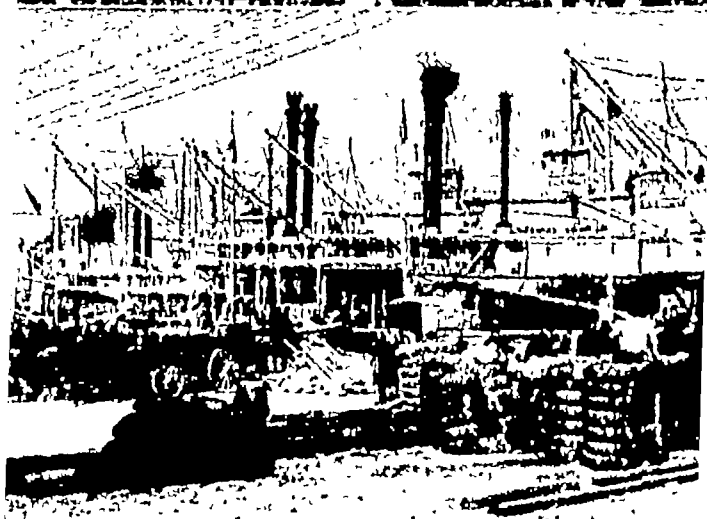
In the small-fruit garden, Burbank developed many highly interesting new forms, some of which are entitled to rank as new species. There is, for example, the Primus berry, a cross between the dewberry and the Siberian raspberry; the Phenomenal berry, a cross between the dewberry and the Cuthbert raspberry; and the Paradox, a cross between the Lawton blackberry and the crystal white blackberry.

Then there are luscious blackberries that are pure white, and others that grow on vines that are as free from thorns as the twigs of an apple tree.

Also there is the sunberry, a palatable fruit produced by combining the traits of two inedible nightshades, and there are numerous new varieties of strawberries, huckleberries, currants, gooseberries, and elderberries, as well as sundry rare exotics that deserve the attention of all lovers of flowers.

In the vegetable garden, Mr. Burbank achieved his earliest success through the production of the Burbank potato, the story of which has already been told. He worked effectively with all the familiar types of garden vegetables, his efforts culminating, perhaps, in the development of the now celebrated crimson winter rhubarb, the ancestor of which came from New Zealand.

Among thousands of experiments with flowers it is hard to choose, so many and so notable were the developments. The Shasta daisy, which combines the strains of species from Europe, from Japan, and from America, has exceptional interest both from a scientific and from a popular standpoint. But scarcely less interesting are the hybridizing experiments through which were produced the giant amaryllis with its nearly twelve-inch blossom, the spectacular tigridias, the scented callas, dahlias, and verbenas, the beautiful watsonias and gladi-



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oli, the wonderfully varied poppies, including one that is blue in color, and the extraordinary colony of lilies showing thousands of new and strange combinations of form and color.

By way of adorning lawn and park, Burbank developed a substitute for grass in the South American lippia which thrives in time of drought, and requires not one-tenth the attention given ordinary lawn grass. He developed a vast number of ornamental shrubs and vines, including new types of clematis with beautiful and varied flowers. And in experimenting with trees he produced walnuts that grow to gigantic size in a few years, and, at the other end of the scale, chestnuts that bear abundant crops when they are mere bushes.

A chestnut that bears large nuts at six months from the seed creates as much astonishment as almost any other single anomaly seen at the famous experiment gardens at Sebastopol.

The chestnut that is developing a smooth burr is also of peculiar interest; matching the walnut that was made to bear so thin a shell that the birds destroyed the nuts, so that it became necessary to thicken the shell by further selective breeding.

These glimpses, together with bare mention of the spineless cactus with its amazing crop of luscious fruit, must suffice to suggest the varied lines of plant experiment that Mr. Burbank carried forward year by year.

A man of Burbank's philosophical cast of mind could not fail to give a vast deal of thought, first and last, to the question of a possible application of knowledge gained in the experiment garden to better development of the human race. In point of fact, Mr. Burbank not only thought but wrote and talked on the subject very extensively. He had very pronounced ideas about the de-

velopment of the human plant that were the outgrowth of his experimental studies with plant life.

Nowadays we all understand that the same general principles apply to all types of living creatures. With the proper allowance for details of variation, the laws of heredity studied in the vegetable garden can be applied with much assurance to the breeding of animals or the betterment of the human race itself. So large a subject cries out for extended treatment, but it is obvious that here I can do no more than make brief reference to the possible application of the principles of plant breeding, as Burbank interpreted them to the human race.

At the very outset, we are met with obvious difficulties. Burbank selected only good stock from which to breed. He saved ten or a dozen plants from a bed of thousands and tens of thousands. Obviously no such restriction is possible in the human family, even were we to put into effect the most sweeping conceptions of the eugenist.

But Burbank optimistically called attention to the fact that the civilized races of today are in effect highly selected stock. They are the result of many centuries of breeding during which society endeavored to rid itself of undesirables. Capital punishment for minor crimes doubtless had an appreciable eugenic influence; and under the pampering conditions of city life, disease decimates the ranks of the weaklings; even wars tend on the whole to remove individuals of less evolved mentality.

So, on the whole, such a stock as the average American race is a highly evolved and selected type, in large measure adapted to its environment, and eminently fit for propagating the species.

But of course some members are better fitted than



others to carry out this function; and at present there is an unfortunate tendency for the better members to have small families while the less desirable ones have large families. It perhaps does not need the advice of the Santa Rosa experimenter to tell us that this propensity, if not checked, must lead to disaster, but his experience may be cited as emphasizing the lesson.

Unless the more desirable members of a race can be made at least as prolific as the less desirable ones, that race must deteriorate.



COTTON

